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CONDITIONING AND THE VOLUNTARY CONTROL OF THE PUPILLARY LIGHT REFLEX 3
By Clarence V. Hudgins

DOES POSITIVE RESPONSE TO DIRECT SUGGESTION AS SUCH EVOKE A GENERALIZED HYPERSUGGESTIBILITY? 52
By Clark L. Hull, Everett F. Patten, and St. Clair A. Switzer

EYE-MOVEMENTS DURING READING AND RECALL . . 65
By Harry Ewert

REINFORCEMENT AND INHIBITION OF EYELID REFLEXES 85
By Ernest R. Hilgard

ON THE RATE OF EXTINCTION OF A CONDITIONED REFLEX 114
By B. F. Skinner

REFLEX AND VOLUNTARY REACTIONS OF THE EYELID 130
By Helen Peak

TWO STUDIES IN RETROACTION: I. INFLUENCE OF PARTIAL IDENTITY. II. SUSCEPTIBILITY TO RETROACTION AT VARIOUS GRADE LEVELS 157
By Thelma A. Dreis

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LEARNING FUNCTIONS IN AN OBSCURE AMNESIA, WITH IMPLICATIONS FOR RE-EDUCATION 173
By F. L. Wells

ON THE NATURE OF "SPEED" AND ITS RELATION TO OTHER VARIABLES 198
By Caroline McCann Tryon

THE EFFECTS ON REACTION-TIME OF VARIOUS ORDERS AND DISTRIBUTIONS OF RELEVANT STIMULI IN THE WORD-ASSOCIATION EXPERIMENT 217
By B. K. Swartz and H. R. Crosland

SHORT ARTICLES AND NOTES 272
- Massed versus Distributed Practice in Pursuitmeter Learning (Walter W. Webb) 272
- Another Short Method for Calculating β Weights and the Resulting Multiple R on a Computing Machine (Cecil R. Brolyer) 278
- A Problem in Meaning (Shigeto Tsuru and Horace S. Fries) . 281

BOOKS 285
- Edward L. Thorndike (and the Staff of the Division of Psychology of the Institute of Educational Research of Teachers College, Columbia), The Fundamentals of Learning (John A. McGeoch) 285
- Books Received 296

54-137
12-7
2

CONDITIONING AND THE VOLUNTARY CONTROL OF THE PUPILLARY LIGHT REFLEX[1]

From the Psychological Laboratories of Clark University

Clarence V. Hudgins

I. Introduction

In the present investigation I have sought to test the possibility of conditioning the pupillary response of the human eye to verbal stimuli and to other stimuli arising from movements under the control of the subject. Cason's 1922 study (1) of the conditioned pupillary response demonstrated that the pupil can be conditioned to the auditory stimulation of a bell. It should be possible, therefore, to condition the pupil to auditory stimuli produced by the verbal responses of the subject himself. If such auditory processes can be established as conditioned stimuli for the pupil, it should be possible to eliminate the auditory components, leaving only the vocal motor responses, and then to reduce these to a subvocal level. The establishment of vocal or subvocal verbal stimuli from the subject as conditioned stimuli for the pupillary response might well give a basis for a better empirical understanding of the nature of that class of behavior which has been called voluntary.

My results confirm the earlier findings of Cason and indicate further that it is possible to condition the pupil to verbal stimuli either from the experimenter or from the subject. Such conditioned responses are independent of the auditory components of the verbal responses which may be vocalized, whispered, or repeated subvocally by the subject.

II. Experimental Section

1. *Apparatus and Subjects.* The apparatus was the same in principle as that of Weiler described and used by Cason (1). It consists essentially of a light source enclosed within a box, a suitable optical system for directing the light into the eye of the observer, a

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[1] The problem was suggested by Professor Walter S. Hunter, and the investigation was carried out under his direction at Clark University in the period from 1930 to 1932.

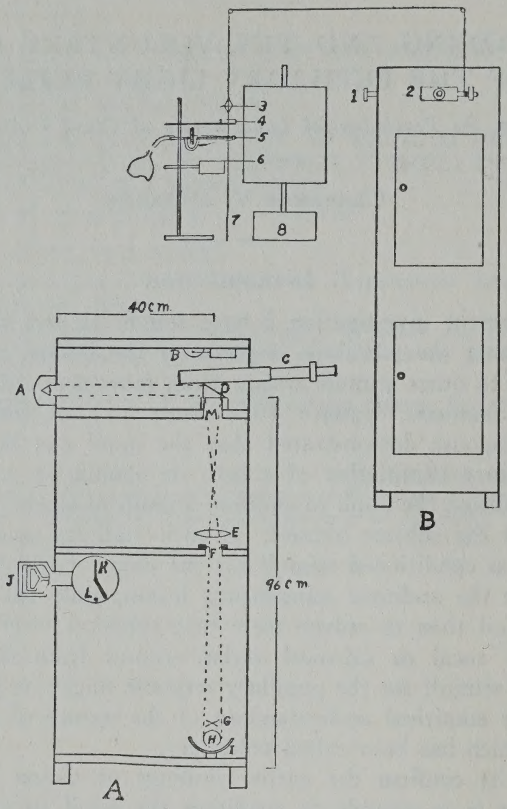

FIGURE 1

THE APPARATUS

A. Side View of Apparatus
- *A*—subject's eye
- *B*—bell
- *C*—telescope
- *D*—right-angle prism
- *E*—lens
- *F*—iris diaphragm
- *G*—fixation point
- *H*—100-watt frosted bulb
- *I*—silvered reflector
- *J*—dynamometer switch
- *K*—light contact
- *L*—bell contact
- *M*—orifice for emission of light through prism

B. Front View of Apparatus Showing Recording System
- *1*—thumb-screw for shifting telescope from left to right
- *2*—micrometer ocular with screw for moving parallel hair
- *3*—marker attached to silk thread for recording changes in diameter of the pupil
- *4*—signal magnet
- *5*—voice recorder
- *6*—Jacquet chronometer
- *7*—rubber band for tension in silk thread
- *8*—kymograph

telescope with a micrometer ocular containing two parallel hairs (one of the latter movable), and a millimeter scale for measuring the diameter of the pupil. (See Figure 1, A and B.)

The light source was a 100-watt Mazda bulb set in a silvered concave reflector at a distance of 136 cms. from the eye of the observer. The bulb was placed in the base of the box enclosure, and the light passed through a lens which focussed it on a 90° prism. From here the light passed into the eye of the observer. The intensity of the light was controlled by an iris diaphragm which was placed immediately beneath the lens. By means of a lever the experimenter could change the size of the diaphragm orifice from a pin-point to a diameter of 36 mms.

Two intensities of light were used in the experiments. The bright training light was the maximum intensity emitted by the 100-watt bulb through an orifice of 28 mms. The test light came from the same source, but was emitted through an orifice of 4 mms. The photometric measurement of these two lights were as follows: training light, 37.7 meter candles; test light, 3.5 meter candles. The training light was used throughout all the conditioning procedures, and the test light was used in making all tests.

A fixation object, consisting of two fine cross wires, was placed immediately above the light bulb. As one looked into the apparatus, from the observer's seat, the light appeared as a brilliant round disc on which was superimposed the fixation cross dividing the disc into quadrants. As the orifice of the diaphragm was made larger or smaller the intersection of the two wires remained in the center of the disc.

Observations and measurements were made upon the pupil by means of a Bausch and Lomb telescope fitted with a micrometer ocular. The instrument was mounted on the front of the apparatus at a point immediately in front of the subject and slightly above his line of regard. The telescope magnified the object five diameters. The ocular contained two parallel hairs set perpendicular to a scale which was graduated to read directly in mms. One of the hairs was movable by means of the micrometer screw, the other remained at a fixed point on the scale. The fixed hair and the entire telescope could be moved slightly to the right or left by means of a screw on the mounting. By this means it was possible to bring the parallel hairs tangent to the pupil on either side and to read the horizontal diameter directly from the scale. The micrometer scale could be read in .001 mm. but it was impossible to take readings of the diameter of the pupil with any degree of accuracy beyond .1 mm. The screws by which the hairs were moved were of such a pitch that, with a little practice, the pupillary changes could be followed easily.

The conditioning stimuli used in the experiment were bell sounds, hand contractions and relaxations, and verbal stimuli. The bell was a 7-cm. door-bell operated by a 2-volt storage cell. A damping arrangement prevented the continuation of the tone after the circuit had been broken. The bell was placed in the top of the box immediately in front of the subject and slightly above the horizontal plane.

The hand contractions and relaxations were made in connection with a hand dynamometer which had been converted into a double switch by the simple device of substituting copper strips for the usual indicators. Two copper strips were used, and contact points were attached to the dial of the dynamometer so that two circuits could be closed simultaneously. The contact points could be adjusted on the dial so that any desired degree of tension of the muscles which closed the hand could be used. A tension of 5 and 4 kgs. was used for the male and female subjects respectively. The circuits for the bell and light were arranged so that they could be closed either by the dynamometer switch or by a second set of switches near the experimenter. The latter set could also break the circuits which were closed by the dynamometer.

The kymograph set-up is shown in Figure 1 B. The diagram shows the front of the box enclosure with the kymograph and recording apparatus on the side. A fine silk thread was fastened to the shaft of the micrometer screw (*2*) and was conveyed to a marker (*3*) by means of pulleys. A thin rubber band (*7*) was attached to the opposite side of the marker in order to provide tension for the system. As the micrometer screw was turned the marker moved up or down leaving a tracing on the moving drum. A slight torsion in the thread and rubber band kept the marker in constant contact with the drum. Since only one of the parallel hairs was movable with reference to the scale, only one thread and marker were necessary to record the changing diameter of the pupil. The apparatus was so arranged that the slight shifts of the entire telescope which were required to keep the second parallel hair tangent to the pupil did not affect the length of the thread. The micrometer scale was amplified 60 times when projected upon the kymogram. The kymograph was practically noiseless while running at the relatively slow speed required for these records. Its effects upon the pupil were negligible. A signal magnet recorded the incidence of three of the conditioning stimuli. One circuit was connected with the dynamometer switch; a second, with a key at the left of the subject which he closed at the moment when he gave the subvocal response; and a third, with a key in the bell circuit which the experimenter could close with his foot.

The verbal stimuli from the experimenter were recorded by means of a pneumatic voice key. Time was recorded in seconds by means of a Jacquet chronometer.

The entire apparatus was set up in a dark-room. The subjects were not dark-adapted because a certain amount of light adaptation was necessary in order that the fixation point might be seen. However, the apparatus was carefully shielded against light leakage, and the only light stimulation which the subject received came from the aperture through which he looked. A small flash light, screened from the subject, enabled the experimenter to make his readings of the micrometer scale.

Fourteen subjects were used in the experiment: 3 were graduate students, 2 in psychology, 1 in chemistry (all familiar with the purpose of the experiment); 7 were undergraduate students entirely naïve as to the nature of the experiment; 2 were secretaries naïve as to the nature of the experiment; 1 was a professor of psychology who was entirely familiar with the problem; and 1 was a high-school student naïve as to the problem.

2. *Procedure.* The general plan of the experiment was so organized that a conditioned response could be established in the pupil to a complex stimulus situation and then this complex stimulus could be progressively reduced to a simpler form. The ultimate goal was to substitute for the complex stimulus a verbal process which would be effective even when repeated subvocally by the subject himself. The method by which this plan was carried out was as follows: (1) The pupil was first conditioned to a bell with the light as the unconditioned stimulus. (2) A hand contraction on the part of the subject was next used to close the light and bell circuits. The relaxation of the hand broke the same circuits. Verbal stimuli from the experimenter were used to evoke the hand responses. (3) The bell and the hand responses were finally eliminated leaving only the verbal stimuli from the experimenter as conditioned stimuli. (4) The subjects produced the verbal stimuli vocally, then by whispering, and finally subvocally; and the pupillary responses were measured.

The initial effects of the various stimuli upon the pupil prior to training were determined first. With this done, the training for conditioning was carried out over a period of six days, with an experimental period of 1½ to 2 hours each day. This training was divided into three general steps, each step occupying two experimental periods. In Step I the pupil was conditioned to the bell stimulus. In Step II the pupil was conditioned to a combination of hand responses, the verbal stimuli from the experimenter, and the sound of the bell. In Step III the pupil was conditioned solely to the verbal stimuli from the experimenter. Tests for the nature and amount of conditioning were given at the end of each experimental period. These tests were made exactly as were those for the initial effects of the stimuli prior to training. The procedure for each of these three steps will be given separately, following a description of the procedure for the initial measurements.

a. Initial measurements prior to training. A group of 8 to 10 measurements was made for each stimulus. The following order was followed throughout; tests for the verbal stimuli given by the experimenter; tests for verbal stimuli both vocal and whispered by the subject (auto-commands); tests for the hand contraction and relaxation; and tests for the bell stimulus. Two subjects received in addition tests with subvocal stimuli. The same test light which would be used later in making the test for the conditioning effects was used in all of these measurements.

The subject was seated at the apparatus and instructed as follows:

> "You are to sit quietly with the eye fixated upon the cross hairs and do nothing unless you are otherwise instructed. When the light disappears, sit quietly in position with your eye still and fixated in the general direction of the extinguished light. When the light reappears, fixate the cross hairs and do nothing unless otherwise instructed."

Tests for the effects of the verbal stimuli were made as follows: The light was turned on; and, after the pupil reached a steady state, it was measured. At this point, the experimenter gave the stimulus words "contract" or "relax" in any random order, and the pupil was again measured. The light was then extinguished, and the test was repeated after 10 secs. Sometimes several measurements were taken without extinguishing the light.

The measurements for the initial effects of the "auto-commands" were made in a similar manner. The subject was instructed as follows:

> "When the light is turned on I want you to repeat one of the words 'contract' or 'relax,' but not both during the same trial. Wait until you can see the fixation points clearly, and then repeat one of the words."

The pupil was measured as it reached a steady state after the light was turned on. It was measured again after the subject repeated one of the stimulus words. The light was then extinguished, and after 10 secs. the trial was repeated. If nonsense words were being used, the same method was followed.

In the tests for the initial effects of the hand response the subject was instructed as follows:

> "Now place your right hand on the dynamometer handle. At the command 'contract,' squeeze the handle until the light appears. Keep the light on until you are told to relax. Then relax the hand and sit quietly with the eye in position. Be sure to keep the fixation point at all times while the light is on. If the light is on when the command is given, contract the hand just the same and hold it until you hear 'relax.' If the light does not disappear when you relax the hand sit still with the eye in position."

These tests were really tests of the hand response plus the verbal stimulus, since the former was elicited by the latter in every case. In the tests for the effects of the contraction of the hand, the test light was turned on and the pupil measured as it reached a steady state; then the verbal stimulus was given which was followed by the motor response. No change in the intensity of the light occurred. The pupil was again measured, the command "relax" given, and the light extinguished. In the tests for the relaxation effect, the procedure was slightly different. The hand contraction was used to turn on the light. Then the experimenter, by manipulating a switch, arranged the circuits so that the light would not be extinguished when the hand was relaxed. As it reached a steady state the pupil was measured for the light effect plus the contraction, and it was measured again after the hand relaxed. The light remained the same. The difference between these two measures was taken as the relaxation effect, since all the other stimuli remained constant.

Measurements for the initial effects of the bell were made in a similar manner. The light was turned on and the pupil measured as in the other tests. Then the experimenter rang the bell for 5 secs. and again measured the pupil. Sometimes several measurements were made without extinguishing the light.

Kymographic records of the effects of the various stimuli were made for two subjects. The entire course of the pupillary changes were thus graphically recorded.

b. Conditioning procedure

1) Step I—Bell. The first step of the conditioning process was that of conditioning the pupil to the sound of a bell. The procedure of Cason (1) was followed here with some minor variations. Cason, who used the same light intensity both for the training and the final tests, found that a large number of trials was necessary in order to condition the pupil. After some preliminary experiments, I decided to use a bright light for the training and a much fainter light for the test. The results show that it is possible to condition the pupil to the sound of a bell with about half the number of trials that Cason found necessary. The responses are also larger than those reported by him. It seemed reasonable that the pupil which was already constricted almost to a maximum in the bright training light could not constrict much more when the conditioning stimuli were presented. This type of argument, however, is not applicable to the case when the conditioned stimuli were to produce dilation of the pupil.

The subject was not dark-adapted at the beginning of the training period because the fixation point could not be seen by the subject until the eye became partially light-adapted. For the training, the subject was seated at the apparatus and instructed as follows:

> "The light and bell will both be present. When the light appears, find the fixation point and keep the eye still during the time that the light is on. When the light disappears, keep the eye fixed in the same position as nearly as you can so that it will not be necessary to shift the eye when the light reappears in order to find the fixation point. Do not wink during the time the light is on unless it becomes necessary. Rest periods will be given at intervals."

A typical training trial was as follows: The light and bell circuits were closed simultaneously by the experimenter. The stimuli remained on for a period of 3-4 secs. This was followed by a period of 10 secs. of darkness and silence. Then the trial was repeated.

Rest periods of 3 mins. were given at the end of every 30 trials. It was possible to condition the pupil to the bell stimulus in one period; but since a relatively large number of trials was required, the eye became fatigued by the time the period was over. This fatigue interfered with the final tests. For this reason the training for each of the three steps was divided into two periods. The first day, 125 trials were given. Tests at the end of this period usually showed some conditioning effect, but only a few measurements were made at this time. On the following day the training continued until from 80 to 100 additional trials had been given. The final tests for this step were made at the end of this series of trials. The method of testing for the conditioning effect of the bell was in every respect identical with the method used in the initial measurements. A 5-minute rest period was given at the end of the training series during which time the subject was allowed to stretch, talk, or sit quietly.

2) *Step II—Hand responses and verbal stimuli.* The subject returned, on the day following the final conditioning to the bell stimulus, for a continuation of the conditioning process. Before further training, tests were given for any residual effects of the previous conditioning. Conditioning to the hand responses was then begun. The instructions were as follows:

> "Place the right hand on the dynamometer switch and at the command 'contract' squeeze the handle until the light and bell appear. The movement is not a sudden jerk, but a steady squeeze. When you hear the bell and see the light hold the contraction until you hear the command 'relax.' Keep the eye fixated upon the fixation point while the light is on and do not shift the eye during the period of darkness. Rest periods will be given at intervals."

A typical training trial was as follows: The experimenter gave the verbal stimulus "contract," or the nonsense word which was the signal for hand contraction. The subject responded by squeezing the dynamometer handle until the light and bell stimuli appeared. The contraction was maintained for about 4 secs. At this point the experimenter broke the bell circuit by means of a separate switch and gave the verbal stimulus "relax." Immediately the subject responded by relaxing the hand and thereby breaking the light circuit. The eye was in darkness for 10 secs., and then the trial was repeated. Rest periods of 3 mins. were given at the end of every 30 trials.

On the first day of the conditioning in which the hand responses were used (the third day of the training procedure) 125 trials were given the subject. Tests were made at the end of this period for the effects of the conditioning. The following day the training continued until 200 trials in all had been given, and final tests for this step were then given. Before making these tests a rest of 5 mins. was allowed the subject.

At this stage of the experiment, if the hand responses and the bell were both omitted and the pupil tested with the verbal stimuli alone, the contitioned response would not appear. It was evident, therefore, that further training was necessary before the responses could be elicited by the verbal stimuli alone.

3) *Step III—Verbal stimuli.* In the further training that was found necessary in order to condition the pupillary response to the verbal stimuli alone, the same procedure was followed as in Step II above, except that the bell stimulus was not used at all. The instructions to the subject were the same, with the following addition: "Repeat the verbal commands subvocally each time you hear me give them." A typical training trial used in this step was as follows: At the command "contract" from the experimenter the subject contracted the hand and at the same time repeated the command subvocally. The hand contraction turned on the light. The contraction was held for 4 secs. Then the experimenter gave the command "relax" followed immediately by relaxation of the hand and darkness. The trial was repeated after 10 secs. A rest period of 3 mins. was given after each series of 30 trials.

The training for this step continued for two consecutive experimental periods (the 5th and 6th days of the series). Tests were given at the end of 125 trials. Whether or not the responses were present during these tests, the subjects returned for further training on the following day. The training was continued until 200 trials in all had been given. Since the purpose of the experiment did not include a study of the rate of conditioning, more trials were probably given than were actually necessary to establish the conditioned responses. All of the subjects received approximately the same number of training trials.

A rest period of 5 mins. was given at the end of the training series, and the pupil was tested for the conditioning effects of the verbal stimuli alone. For these tests the subject was instructed to remove the hand from the dynamometer, to leave it relaxed in his lap, and to make no movement when he heard the commands. The test light was turned on by the experimenter, and the pupil was measured as in the initial tests. At this point one of the verbal stimuli was repeated by the experimenter. The pupil was again measured, and the light was extinguished by the experimenter. The testing procedure was varied at this stage. Sometimes a random order was used in the repetition of the stimuli by the experimenter, the light being extinguished after each trial. At other times the stimuli "contract" and "relax" were given, one following the other, at an interval which would allow for the completion of a single response of the pupil.

In addition to tests for the effects of the verbal stimuli from the experimenter, tests were also made, without further training, for the effects of these same stimuli when they were given by the subject himself. The subject was instructed as follows:

> "When the light is turned on wait until you can see the fixation point clearly and then repeat one of the words which I have been using as a signal for the hand contraction or relaxation. Do not repeat both of these words during any one trial. Be sure you can see the fixation point clearly all the time."

The pupil was measured before the subject repeated the stimulus. It was measured again after the verbal stimulus had been given. Tests for the whispered verbal stimuli were given in the same manner. Instructions to the subject were simply changed to include "whispering." Finally the subject was instructed to give the stimuli "subvocally." At this point it became necessary for the experimenter to know at what point the subvocal stimulus was given. Since the stimulus could not be heard by the experimenter, the subject was instructed to tap lightly on the table with the left hand when he gave the stimulus. One tap signalled the stimulus for contraction, two taps signalled the stimulus for relaxation.

Where kymographic records of the tests for the subvocal stimuli were secured the point of stimulation is shown by a rise or fall in the tracing of the signal magnet. The subject simply pressed a key with the left hand at "contract" and released the key at "relax." Sometimes the subject was instructed to repeat subvocally "contract" and to follow it 10-15 secs. later with "relax," the light remaining constant meanwhile.

3. *Results.* The results of the experiment will be presented separately for each step of the conditioning process. The data are of two kinds: (1) the actual measurements of the changes in the diameter of the pupil when the various stimuli were presented; and (2)

the kymographic records which show the temporal course of the response. Complete kymographic records were taken on only two subjects. The method was adopted after the work was well under way, and most of the subjects had completed their training before the method was introduced.

The non-kymographic data are presented in terms of the difference between the diameter of the pupil in a constant light (test light) and the diameter after the conditioned stimulus had been presented. The values, therefore, represent the change in horizontal diameter of the pupil while keeping the light constant and varying the other stimuli. The distribution curves represent the distribution of these values, both before and after the pupil had been conditioned. The distribution to the left of zero is the pupillary response to the conditioned stimulus before training, and the distribution to the right of zero is the pupillary response to the same stimulus after conditioning. The only exception to this general rule is in those distributions which represent conditioned dilation of the pupil. In this case both distributions are to the left of zero. The distance between the means of any two distribution curves represents the average total effects of the conditioned stimuli after training. The units on the base lines are given in 10th mm., and the units on the ordinates represent the actual frequencies of the responses. The zero point on the base line is the point at which no change of pupil occurred when the conditioned stimulus was presented. These distribution curves are based on the records of 14 subjects.

a. Conditioned responses to the bell. Before training, the sound of the bell was followed in every case by dilation of the pupil. Table 1, Column 1, gives the average results of these initial tests for each subject separately. These averages are based upon 6-10 measurements for each subject. It will be observed that the range of these averages is from .36 to .80 mm. This means that prior to the training the sound of the bell was followed by a dilation of the pupil to the extent which is represented by these values. The average initial dilation for the entire group of 14 subjects is .57, *S.D.* .18 mm. Figure 2 is a kymographic record of the course of one of these dilation responses. The bell stimulus began at the point indicated by a rise of the tracing made by the signal magnet, *M,* and continued to sound until the tracing dropped. The course of the dilation, as followed by the experimenter with the parallel hairs of the micrometer, shows that the response occurred in an intermittent manner. The average latent time of this response, as determined from a number of records, is 1.01 secs. This value is subject to the same error as are all of the kymograms. The experimenter's own reaction-time is present; but since this may be considered as remaining relatively constant throughout, the latency values are given just as they appear in the records. The dilation response continued for several seconds. When the bell ceased the pupil contracted approximately to its former diameter. When no stimulus other than the test light was presented no such changes in the pupil occurred.

In contrast to the unconditioned dilation response shown in Figure 2 is the conditioned contraction response (for the same subject) to the bell, shown in Figure 3. The incidence of the stimulus is shown by a rise of the signal magnet tracing, *M*. The contraction of the pupil is shown by the descent of tracing *P*. Some interesting facts

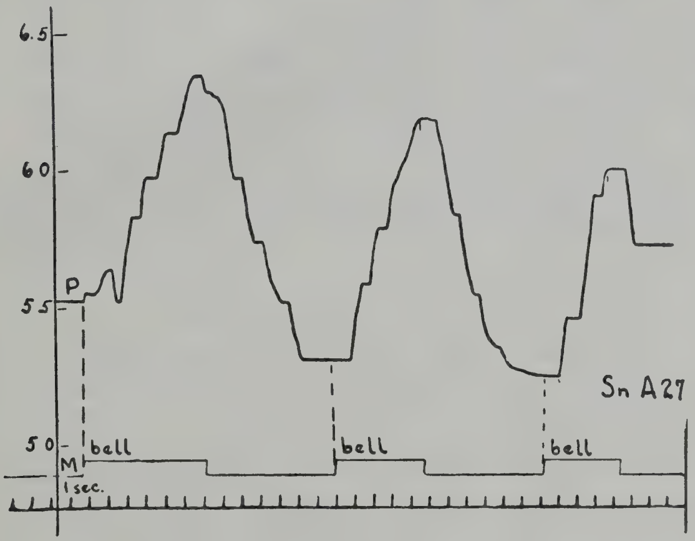

FIGURE 2

Kymogram of Pupillary Response to the Bell Stimulus before Training

P—tracing of pupillary changes. The vertical broken lines connect simultaneous points on the tracings *P* and *M*. A rise in the tracing *P* indicates dilation; a fall, contraction. The test light was on continuously during the record.

M—tracing of signal magnet. The upper levels of *M* indicate the presence of the bell sound; the lower levels, the presence of the test light alone. Time in seconds; ordinate in millimeters.

TABLE 1

AVERAGE EFFECTS OF TRAINING TOWARD CONTRACTION WITH THE VARIOUS STIMULI

	Before training Stimuli					After training Stimuli				
Subject	B.	H.C.	V.C.	A.C.	Sv.R.	B.	H.C.	V.C.	A.C.	Sv.R.
Tapn.	.41	.43	—	—	—	—.61	—.08	—.52	—	—
Smt.	.55	.61	.22	—	—	—.67	—.20	—.73	—.91	—
Lght.	.52	.51	.39	—	—	.00	.00	—.20	—.39	—
Hdgs.	.80	.59	.18	.33	—	—.23	—.42	—.56	—.51	—.64
Hnt.	.49	.40	.19	.28	—	—.14	—.21	—.21	—.38	—.65
Ktl.	.52	.43	.19	.42	—	—.47	—.60	—.77	—.80	—1.56
Fshm.	.80	.63	.53	.69	—	—.49	—.41	—.80	—.64	—
Kmp.	.66	.42	.50	.45	—	—.25	—.55	—.47	—.34	—
Bkv.	.58	.47	.28	.50	—	—.37	—.50	—.52	—.81	—
Mkp.	.56	.49	.49	.50	—	—.37	—.42	—1.20	—.85	—.80
Vkp.*	.52	.45	.46	.44	—	—.40	—.32	—.89	—.71	—
Jffe.**	.36	.57	.30	.32	—	.93	.80	.85	.85	—
Gce.	.64	.51	.53	.54	.43	—.58	—.52	—.99	—.80	—.97
Snpe. *	.58	.39	.52	.60	.33	—.45	—.90	—.74	—1.07	—
M.	.57	.55	.37	.47	.38	—.31	—.32	—.67	—.71	—.81
S.D.	.18	.17	.19	.14	—	.31	.32	.32	.32	.39
No.	117	76	77	100	19	197	186	168	100	32

*Nonsense words substituted for "relax" and "contract" with this subject.
**Subject conditioned to dilate the pupil with these conditioning stimuli.
B.—bell
A.C.—auto-"contract"
H.C.—hand contraction
Sv.R.—subvocal "contract"
V.C.—verbal "contract" from experimenter

appear in this record. The latency of the response is 2 secs. This is ten times as long as the latency of the light reflex as found by Weiler (10, p. 161). A second fact which appears in this record is the relatively slow time of contraction. The times of contraction for the two responses shown in Figure 3 are 12 and 9 secs. respectively. The tracing for the light reflex, if it could be recorded at all by this method, would drop very rapidly on a drum moving at this rate. A third fact, which is obvious from the first of the two responses shown in the record, is that the pupil, at the sound of the bell, started to dilate much in the same manner as it did before training. This movement was short and rather difficult to follow with the apparatus, but it appears in nearly every record. Only a slight indication of it appears in the second response shown in Figure 3. After the slight dilation, however, the direction of the movement was changed. The

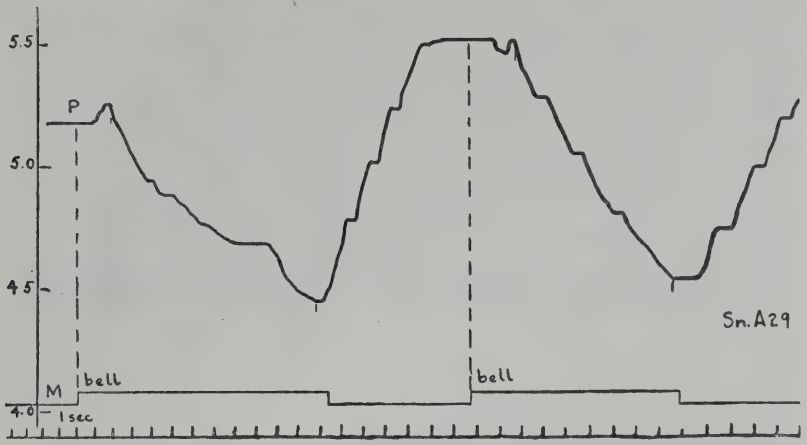

FIGURE 3

KYMOGRAM OF CONDITIONED PUPILLARY RESPONSE TO BELL STIMULUS

P—tracing of the pupillary changes. Broken lines connect simultaneous points on *P* and *M*. Two seconds after the bell rings the pupil begins to constrict. There is a slight dilation immediately following the stimulus. When the bell ceases to ring dilation of the pupil follows.

M—tracing of the signal magnet. The rise of the tracing represents the incidence of the bell stimulus; the tracing falls when the bell ceases to ring. The scale at the left is a projection of the millimeter scale of the micrometer.

Time in seconds. (Compare with Figure 2.)

pupil began to constrict in an intermittent manner. The final fact revealed by the record is that the pupil began to dilate at once when the bell ceased to ring. This is a complete reversal of the response which appeared in the record (Figure 2) taken prior to the conditioning.

Figure 4A is a distribution curve showing the results of the conditioning to the bell. The data to the left of zero represent the initial effects of the bell, and the data to the right are the effects of the same stimulus after training. The means and standard deviations are drawn in the figure. It is very clear that the curve represents two definite distributions. The total effect of the conditioned stimulus is represented by the distance on the base line between the means of the two distributions. The averages for each subject both before and after training are given in Table 1, Columns 1 and 6.

b. Conditioned responses to the hand reactions plus the verbal

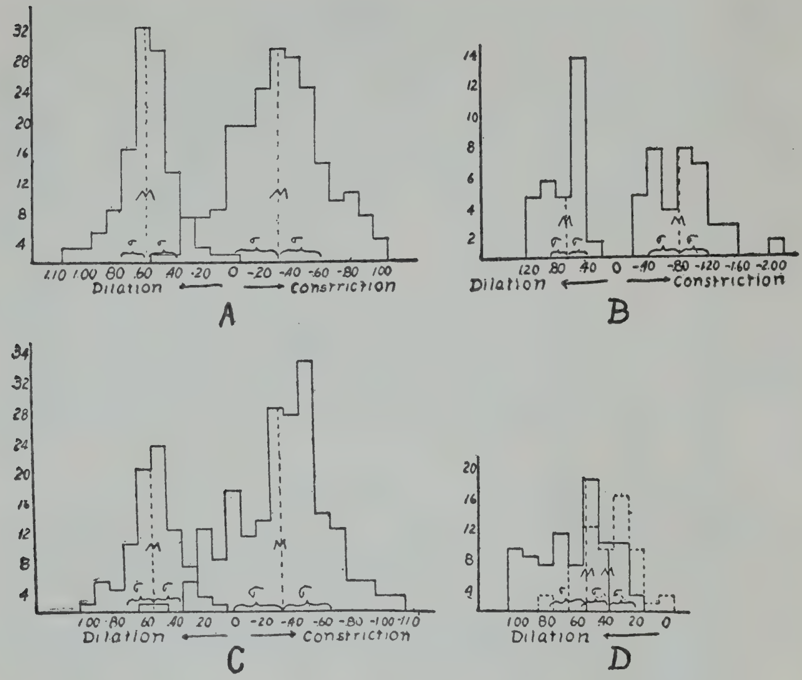

FIGURE 4

Distribution Curves Showing Pupillary Responses (Changes in Diameter) to Various Stimuli before and after Training

A—distributions of responses to bell. The curve to the left of zero shows the responses before training, that to the right shows the responses after training.

B—distributions of responses to subvocal commands after training. The curve to the left of zero shows the dilation responses to "relax," that to the right shows the constriction responses to "contract."

C—distributions of responses to the hand contraction. The curve to the right of zero shows the dilation responses before training, that to the right shows the constriction responses after training.

D—distributions of responses to the hand relaxation. Both distributions are to the left of zero indicating dilation of the pupil. The broken line shows the dilation responses before training, the full line shows the dilation responses after training.

The values on the base line represent .10 mms.

The means and *S.D.*'s are represented graphically in the curves. The distance between the means of any two curves (except in *B*) represents the total average effects of the conditioning process.

stimuli. Like the bell, the hand contractions and relaxations were followed by dilation of the pupil prior to training. Table 1, Column 2 shows the average extent of this dilation for each subject separately. The range for the group of subjects is from .39 to .63 mm. The average for the entire group is .55, *S.D.* .17 mm. A kymograph record of this unconditioned response is shown in Figure 5. The verbal stimulus (from the experimenter) which elicited the hand movement is recorded in tracing *V*. The contraction of the hand of the subject followed immediately. The rise of tracing *P* indicates the dilation of the pupil. The scale on the left of the figure indicates

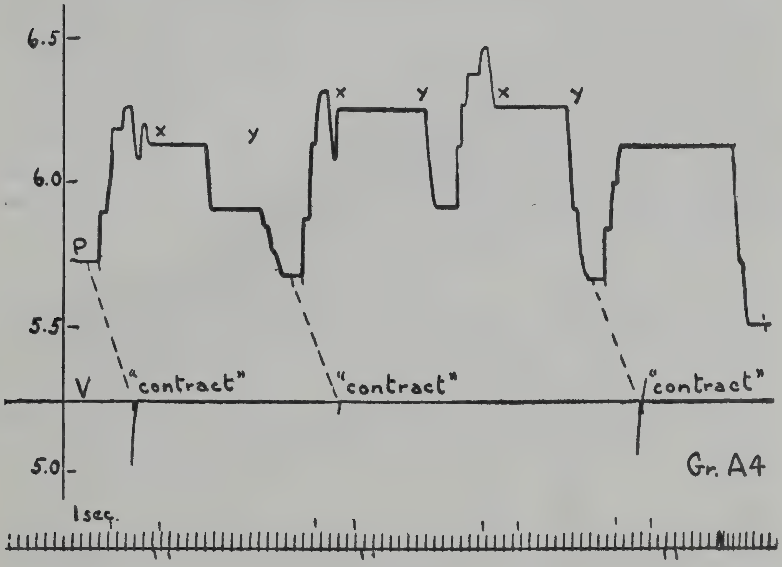

FIGURE 5

Kymogram of the Pupillary Response to the Hand Contraction and the Verbal Stimulus "Contract" Prior to Training

P—tracing of the pupillary changes. The portion of the tracing between *x* and *y* is not representative of the movements of the pupil. During this period the light was out and the apparatus was being adjusted for the next measurement. The broken line connects simultaneous points *P* and *V*. The pupil dilates, after a brief latency, to verbal stimulus and hand contraction.

V—tracing of the voice recorder. The verbal stimulus is represented by a drop in the tracing. The hand movement which followed the verbal stimulus is not recorded.

Time in seconds; ordinate in millimeters.

the extent of the response in mms. Simultaneous points on tracings *P* and *V* are connected by broken lines. After a brief latent period the pupil dilated. As the dilation reached its maximum the light was extinguished. The portions of the tracing between *x* and *y* are not representative of the movements of the pupil. During this interval the light was out, and the apparatus was being adjusted for the next test. There was no essential difference between the magnitude of the dilation produced by hand contraction and that caused by hand relaxation. (Table 1, Column 2 and Table 2, Column 1.)

Two different responses were established in the pupil during Step II. At a given verbal stimulus, by previous instruction, the hand contracted. At another verbal stimulus the hand relaxed. These two responses on the part of the subject were used to operate the light and bell during this portion of the training. At the end of the training, tests showed not only that the pupil constricted when the hand contracted but also that relaxation of the hand was followed by dilation of the pupil to a greater extent than it was during the initial measurements. (See Table 2, Columns 1 and 4.) Data for 11 subjects were available for this test.

TABLE 2

Average Effects of Training toward Dilation with the Various Stimuli

	Before training Stimuli				After training Stimuli			
Subject	H.R.	V.R.	A.R.	Sv.R.	H.R.	V.R.	A.R.	Sv.R.
Hdgs.	.33	.21	.35	—	.36	.47	.48	.60
Hnt.	.25	.18	.35	—	.30	.24	.46	.40
Ktl.	.39	.14	.43	—	.38	.52	.50	.63
Fshm.	.75	.57	.70	—	.70	.72	.86	—
Kmp.	.12	.46	.43	—	.48	.63	.70	—
Bkv.	.30	.32	.22	—	.62	.62	.64	—
Mkp.	.43	.50	.52	—	.75	.77	.65	.50
Vkp.*	.44	.46	.42	—	.69	.94	.86	—
Jffe.	—	.30	—	—	—	.85	—	—
Gce.	.50	.53	.54	.43	.68	.86	1.05	1.07
Snpe.*	.39	.52	.60	.33	1.00	.76	1.00	—
M.	.39	.38	.48	.38	.52	.66	.73	.66
S.D.	.18	.20	.14	—	.23	.26	.26	.23
No.	51	101	96	19	76	134	62	27

*Nonsense words substituted for "relax" and "contract" for these subjects.

H.R.—hand relaxation *A.R.*—auto-"relax"

V.R.—verbal "relax" *Sv.R.*—subvocal "relax"

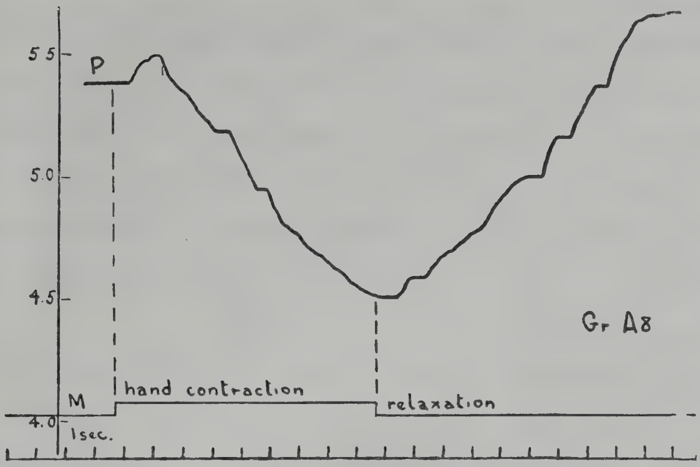

FIGURE 6

Kymogram of the Conditioned Pupillary Response to the Hand Contraction and Relaxation After 125 Training Trials with Bell, Verbal Stimuli, and Hand Responses

P—tracing of the pupillary changes. Downward direction represents constriction; broken lines connect simultaneous points on *P* and *M*, i.e., incidence of stimuli. The initial rise or dilation of the pupil was followed by constriction response. As the hand relaxed the tracing rose to a new high level. There was no change in the illumination during this record.

M—indicates the duration of the hand contraction. The upper level of the tracing indicates the duration of the hand contraction.

Time in seconds; ordinate in millimeters. (This record is to be compared with Figure 5.)

Figure 6, to be contrasted with Figure 5, is a kymogram of the conditioned response of the pupil to both hand contraction and relaxation for the same subject. The signal magnet tracing, *M,* rises at the incidence of the hand contraction. The broken line connects simultaneous points on tracings *P* and *M*. The relaxation of the hand is indicated by a drop in *M*. In Figure 6 there is a latency of about 2 secs. for contraction. (See Table 4 for average latency records.) The latency is slightly less for the dilation response. There was a slight dilation of the pupil immediately following the incidence of the contraction stimulus which is characteristic of nearly all of the records. The course of the movement was quickly changed, however, and the constriction response continued for 9 secs. When the hand relaxed the pupil reversed its action and began to dilate. This response continued for nearly 10 secs., and the pupil dilated to a point which is greater than the point from which the original constriction started.

Figure 4C is a distribution curve for the responses to the hand *contraction* both before and after conditioning. The distribution to the left of zero gives the unconditioned dilation, and the distribution to the right represents the conditioned constriction responses to the hand contraction. The total average effects of the conditioning process is measured by the distance between the means of the two distributions. Table 1, Columns 2 and 7, gives the individual averages before and after training for 14 subjects.

Figure 4D shows the distribution of pupillary changes before and after training when the hand relaxation and the command "relax" were used as stimuli. All of the data are distributed to the left of zero, indicating that dilation occurred in every case. There is also much overlapping in the two curves. There is a reliable difference between the means of the two distributions, however, as is indicated by the fact that $Diff./\sigma_{diff.}$ equals 3.47. Table 2, Columns 1 and 5, gives the individual averages before and after training for 11 subjects.

c. Conditioned responses to the verbal stimuli. The process of conditioning the pupil to the verbal stimuli was begun in Step II above. Additional training was necessary, however, before the pupil was conditioned to the verbal stimuli alone. Prior to training, these stimuli produced dilation of the pupil. Table 1, Column 3, and Table 2, Column 2, show the extent of this initial dilation in terms of individual averages. Both "relax" and "contract" were followed by dilation of the pupil in equal amounts. From Table 1, Column 3, it appears that the initial response to "contract" has a range of .18 to .53 mm., while the group average is .37, *S.D.* .19 mm. From Table 1, Column 8, the individual averages for the same command after training range from —.21 to —1.20. The same verbal stimulus which produced dilation before training produced contraction after training. The average constriction for the entire group is —.67, *S.D.* .32 mm.

Figures 7 and 8 are kymographic records of pupillary responses to the verbal stimuli alone. Figure 7 shows the response prior to conditioning. The presentation of the verbal stimulus is indicated by a drop in tracing *V*. Broken lines connect these points with simultaneous points on tracing *P*. After a brief latent period the pupil dilated as is indicated by the rise in tracing *P*. The light was extinguished as the dilation reached a maximum and the tracing from this point, *x*, to a point, *y*, is not the tracing of the pupil. (The apparatus was being adjusted for the next test.) Figure 8 is a record for the same subject of the pupillary responses to the verbal stimuli after training. There was a slight dilation immediately after the stimulus "contract" was given. This was followed by a series of short constrictions which reduced the diameter of the pupil slightly more than 1 mm. During this response of the pupil, which continued for 13 secs., there was no

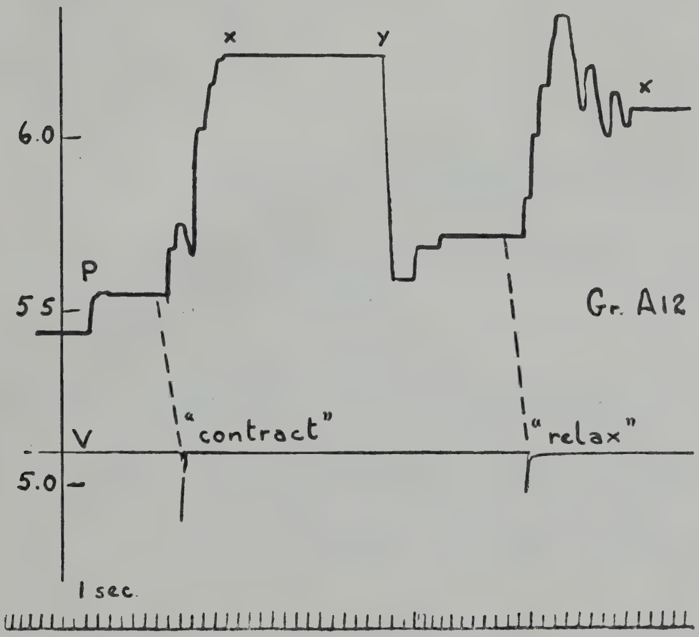

FIGURE 7

Kymogram of Pupillary Response to Verbal Stimuli before Training

P—tracing of pupillary changes. The incidence of the stimulus is represented by broken lines connecting tracings *P* and *V*. Dilation of the pupil followed both of the verbal stimuli. The portion of the tracing between *x* and *y* is not representative of the movement of the pupil since the apparatus was being adjusted for the next test while the light was off.

V—tracing of voice recorder. A drop in the line represents the verbal stimuli.

Time in seconds; ordinate in millimeters.

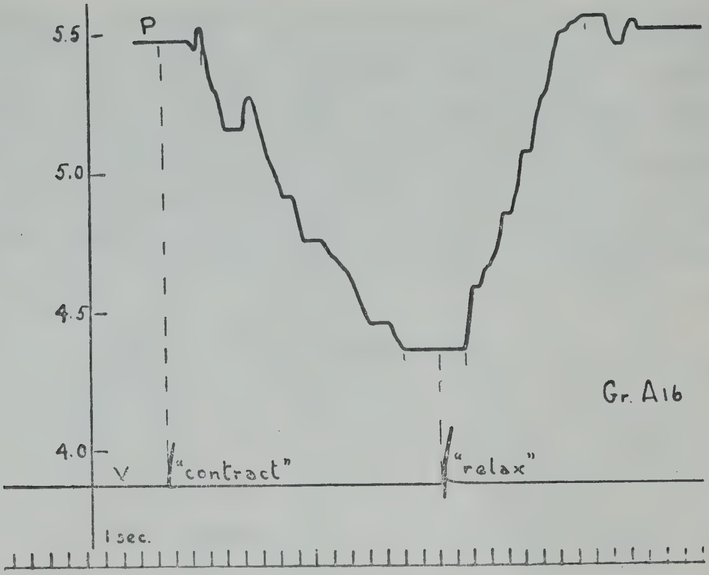

FIGURE 8

Kymogram of Conditioned Pupillary Constriction and Dilation to Verbal Stimuli after Training

P—tracing of pupillary changes. The broken lines connect tracings *P* and *V* at simultaneous points, i.e., incidence of verbal stimuli. The tracing descends in an intermittent manner after a latency of over 2 secs. The slight initial rise immediately following the stimulus represents a trace of the sensory reflex. Dilation followed the stimulus "relax." The actual extent of change in the pupillary diameter was 1 mm.

V—tracing of voice recorder. A rise in the tracing marks the incidence of the verbal stimuli. No hand contraction was used.

Time in seconds; ordinate in millimeters. (To be compared with Figure 7.)

change in the light intensity. At the end of the constriction the command "relax" was given as indicated by tracing *V*. Dilation followed and continued for a period of 9 secs. At the end of this response the pupil had a greater diameter than in its original response to the light alone. The total extent of this dilation movement is 1.3 mms. The irregularity of the movements is indicated by the unevenness of the tracing.

Figure 9A is a distribution curve which shows the result of conditioning to the verbal stimulus "contract." The data to the left of

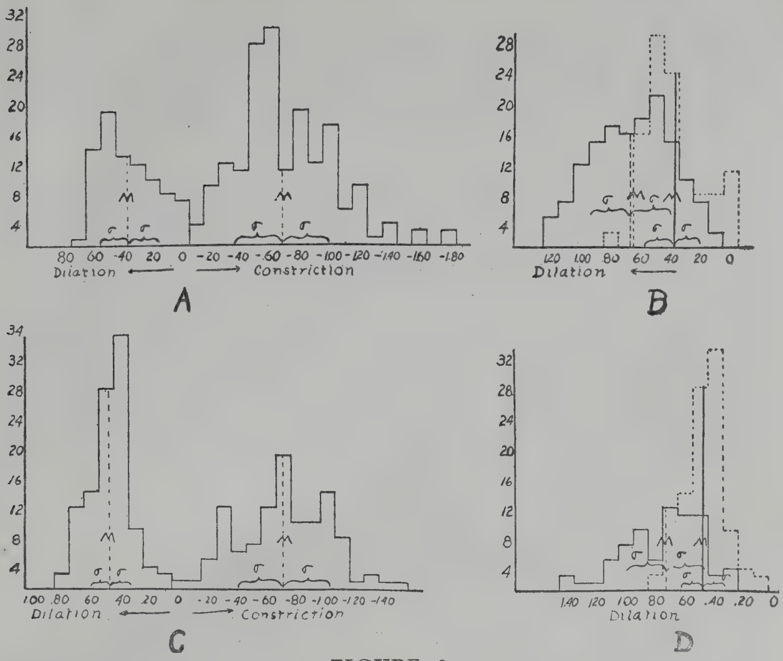

FIGURE 9

Distribution Curves Showing Pupillary Responses (Changes in Diameter) to Various Stimuli before and after Training

A—distributions of responses to verbal stimulus "contract" from the experimenter. The curve to the left of zero shows the dilation responses before training, that to the right shows the constriction responses after training.

B—distributions of responses to verbal stimulus "relax" from the experimenter. Both distributions are to the left of zero indicating dilation in every case. The broken line shows the responses before training, the full line shows the responses after training.

C—distributions of the responses to the auto-command "contract" repeated aloud and whispered by the subject. The curve to the left of zero shows the dilation responses before training, that to the right shows the constriction responses after training.

D—distributions of responses to the auto-command "relax" repeated aloud and whispered by the subject. Both distributions are to the left of zero, indicating dilation of the pupil. The broken line shows the responses before training, the full line shows the responses after training.

The values on the base line represent .10 mms.

The means and *S.D.*'s are represented graphically in the curves. The distance between the means of any two curves represents the total average effects of the conditioning process.

zero represent the responses before training, and the data to the right represent the responses after training. The effects of the training are clearly shown in these curves. The total average effect is represented by the distance on the base line between the two means of the distributions.

The evidence is clear in the case of the verbal stimuli that there were two types of conditioned responses being formed simultaneously. One was a conditioned constriction, the other was conditioned dilation. The pupil "learned to discriminate" between the two verbal stimuli, "contract" and "relax," in the same manner that it "discriminated" between the hand contraction and the hand relaxation. Table 2, Column 2, gives the average initial effects of the stimulus "relax" for 11 subjects. The range is from .14 to .57 mm. The average for the group is .38, *S.D.* .20 mm. The table (Column 6) also shows the effects of the same stimulus after training. Here the

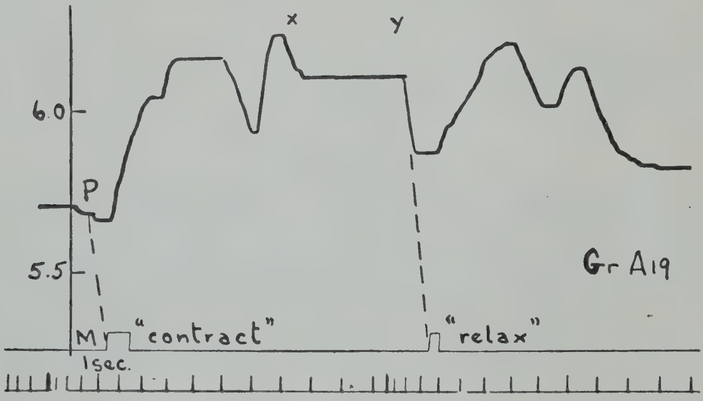

FIGURE 10

KYMOGRAM OF PUPILLARY RESPONSE TO VERBAL STIMULI REPEATED BY SUBJECT HIMSELF BEFORE TRAINING

P—tracing of pupillary changes. Simultaneous points are connected by broken lines between *P* and *M*. The portion of tracing between *x* and *y* is not representative of movements of the pupil; the light was out and the apparatus was being adjusted for the next test. There is a rise in tracing immediately following the verbal stimuli. The kymograph was stopped between tests.

M—tracing of signal magnet. The subject closed the circuit just as he repeated the verbal stimulus.

Time in seconds; ordinate in millimeters.

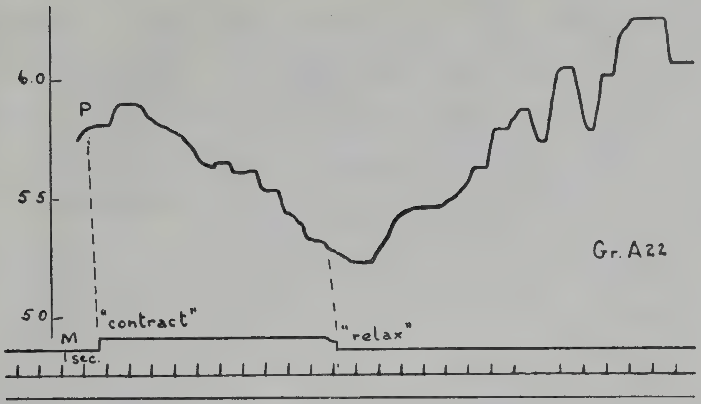

FIGURE 11

Kymogram of Conditioned Pupillary Responses to "Auto-verbal Stimuli"

(Same subject as shown in Figure 8, no additional training.)

P—tracing of pupillary changes. Simultaneous points are connected on tracings *P* and *M* by broken lines, i.e., incidence of stimuli. The subject pressed the key as he repeated the word and held it down during the pupillary response; he released the key on "relax" and dilation of the pupil followed. The unevenness of tracing indicates the intermittent nature of the response.

M—tracing of signal magnet. This shows the incidence and duration of the stimulus.

Time in seconds; ordinate in millimeters. (To be compared with Figure 10.)

range is from .24 to .94 mm. The group average is .66, *S.D.* .26 mm. Figure 9B shows the two distributions plotted in the usual manner. The data all fall to the left of zero since the responses were dilations. There is much overlapping in the two curves, but the difference between the two means is very reliable since $Diff./\sigma_{diff.}$ equals 9.18.

d. Conditioned responses to the auto-verbal stimuli. The results reported in the preceding section indicate clearly that verbal stimuli from the experimenter can take the place of the original total complex stimulus. As a method of further verifying the adequacy of the verbal stimuli to elicit the conditioned response, the subjects themselves were now required to repeat aloud the verbal stimuli. Tests for the effects of these auto-commands were given, without further training, immediately after the tests reported above. Prior to any training, tests had shown that auto-commands were followed by dilation of the pupil (Table 1, Column 4, and Table 2, Column 3). Figure 10 is a record of such an initial dilation. The signal magnet tracing, *M,* indicates the point at which the subject repeated the words. The broken lines indicate the simultaneous points on tracing *P.* (The light was extinguished between tests, therefore, the tracing between the points *x* and *y* is not the true course of the pupil.) The subject closed the circuit of the signal magnet by pressing a key with his left hand as he repeated the verbal stimulus.

In contrast with Figure 10, Figure 11 presents a kymographic record of the *conditioned* responses to the auto-commands. The familiar responses are apparent. The tracing of the signal magnet, *M*, marks the incidence of the stimuli. The key was pressed at "contract" and held until "relax" was repeated. Simultaneous points are connected on tracings *P* and *M* by the broken lines. The pupil contracted when the subject said "contract" and dilated when he said "relax."

Figures 9 C and D are distribution curves of the data obtained in the tests for the effects of the auto-commands before and after training. These curves represent both vocal and whispered repetitions of the verbal stimuli by the subjects. Figure 9 C is the distribution of the responses to "contract," and D shows the distribution of the responses to "relax." The mean and standard deviations are represented graphically in the figures. The total average effect of the auto-command "contract" is represented by the distance on the base line between the means of the two distributions. The distribution to the left of zero represents the data before training, and the distribution to the right represents the data after training. In Figure 9 D the data are all distributed to the left of zero. The difference between the two means in Figure 9 D is a reliable one as indicated by the fact that $Diff./\sigma_{diff.}$ equals 6.8.

A possible criticism might be raised at this point to the effect that the slight movement made by the subject in pressing the signal key may have been the active stimulus in eliciting the pupillary response, instead of the verbal stimuli given by the subject. This is improbable, however, for the following reasons: (1) The key was used only with the 2 subjects for whom kymographic records were made, while the same pupillary responses to the auto-verbal stimuli above described were observed directly on 12 subjects. (See Tables 1 and 2, Columns 4 and 9, 3 and 7, respectively.) (2) It is unlikely that the same movement would be an adequate stimulus for both constriction and dilation in consecutive tests on the same subject. (3) In Figure

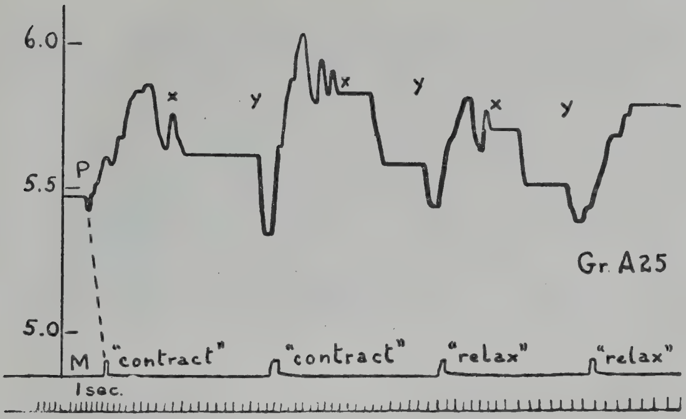

FIGURE 12

KYMOGRAM OF PUPILLARY RESPONSES TO SUBVOCAL REPETITION OF VERBAL STIMULI BEFORE TRAINING

P—tracing of pupillary changes. The broken line connects simultaneous points on tracings *P* and *M*. That portion of the tracing between *x* and *y* is not representative of the pupillary changes; the aparatus was being adjusted while the light was off.
M—tracing of signal magnet. The subject pressed the key as he subvocally repeated the verbal stimuli.
Time in seconds; ordinate in millimeters.

14 (discussed below) the pupil was not materially affected by the movement of pressing the key when no other stimulus except the test light was present.

e. Conditioned responses to subvocal stimuli. The conditioned responses were tested finally by having the subject give himself the verbal stimuli subvocally. The results indicate that the responses can be evoked independently of the auditory components of the verbal stimuli. Initial measurements for the subvocal responses were made on only two subjects, but, after training, tests were made with five subjects. The average dilation for the 19 initial measurements, Table 2, for the two subjects is .38 mm. The average constriction for the five subjects after training as shown in Table 1, Column 10, is .81 mm. Figure 12 is a kymogram of the initial effects of the subvocal stimuli before training. The signal magnet indicates the points of stimulation. There was a slight dilation of the pupil in each instance before training began. The light was extinguished between tests. Figure 13 is a kymogram showing the response of the same subject's pupil to these stimuli after training. The subvocal tests were made immediately following the tests with the vocalized and whispered auto-verbal stimuli. The subject was instructed to sit quietly and repeat "to himself" the command "contract," to wait about 15 secs., and then to repeat in a similar manner "relax." The key (tracing *M*) was pressed at the moment the stimulus "contract" was given, held down during the interval, and released at "relax." The record shows that after training the subvocal stimulus "contract" produced a pupillary constriction and the stimulus "relax," a pupillary dilation.

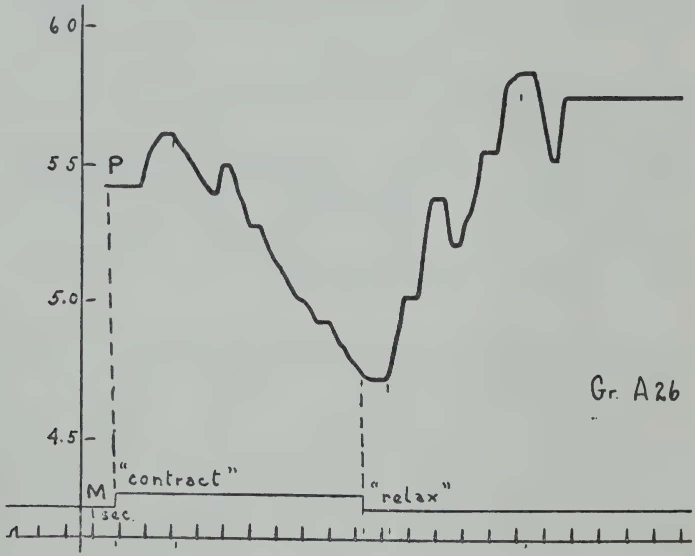

FIGURE 13

Kymogram of Conditioned Pupillary Responses to Subvocal Verbal Stimuli

P—tracing of pupillary changes. Simultaneous points on tracings *P* and *M* are connected by broken lines, i.e., the incidence of the stimuli. There is a slight dilation of the pupil immediately following incidence of the stimulus; this was followed by constriction. When the subject repeated subvocally "relax" the pupil dilated.

M—tracing of signal magnet. The subject pressed the key as he repeated the stimuli.

Time in seconds; ordinate in millimeters. (To be compared with Figure 12.)

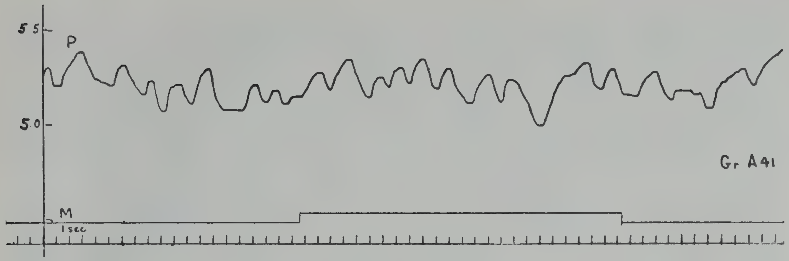

FIGURE 14

Kymogram of Pupillary Changes When the Subject Presses the Key; No Other Stimulus Presented

P—tracing of the pupillary changes. Slight "tremor" movements, .10 to .25 mms. in amplitude, and at the rate of 1 in 2 secs., are apparent throughout the record. There are only slight disturbances of the rhythm and amplitude when the subject presses and holds the key. The test light was used for this test.

M—tracing of the signal magnet. The subject was instructed to press and hold the key down during some portion of the record. The upper portion of the tracing represents the time that the key was being held down.

Time in seconds; ordinate in millimeters.

Figure 4 B is a distribution of data from five subjects who were tested for the presence of the conditioned responses to the subvocal stimuli. Each of these curves represents the data for the post-training tests. The distribution to the right of zero represents the conditioned responses to the subvocal "contract," and the distribution to the left represents the conditioned responses to "relax."

It is necessary at this point to consider briefly all the stimuli which may have been present when the auto-commands were used to evoke the conditioned pupillary responses. It is possible that factors other than those directly involved in the verbal responses of the subject were responsible for the pupillary changes. The probable stimuli involved were: (1) the test light, (2) the finger movement in pressing the key or tapping the table, (3) accommodation-convergence responses, (4) the auditory components of the verbal stimuli, (5) possible contractions of the hand (either implicit or overt) which had been evoked by the verbal stimuli throughout the conditioning procedure, (6) the neuro-muscular set which was induced by the instructions to the subject, and (7) other motor results of the verbal stimuli. These possible factors will be considered in the above order: (1) The test light can be eliminated as a causal factor in the conditioned response since it remained constant, and the observations were made on the pupil after the latter had reached a steady state in response to this light. (2) It is possible to test for the effects of the finger movement by recording the pupillary reactions when the subject is instructed simply to press the key at any given moment and to hold it down without repeating the verbal stimuli subvocally. Figure 14 is a record of such a test. The pupil (tracing *P*) shows slight regular oscillations throughout, and it is only slightly disturbed when the key is pressed and released (rise and fall of tracing *M*). There are no gross changes such as are apparent in Figure 13. (3) In view of experiments to be described later the accommodation-convergence mechanism can also be dismissed. (See results of tests, pp. 40-41 below.) (4) The auditory components of the verbal stimuli can also be eliminated as a necessary factor because the responses could also be produced by the self-presentation of *subvocal* stimuli. (5) The contraction of the hand cannot be definitely ruled out as a stimulus factor since it is impossible to say that no contractions occurred. If such contractions occurred, however, either overtly or implicitly, they were evoked by the verbal stimuli themselves and were a part of the response aroused by the latter. I have no evidence regarding the action of the hand during the tests with auto-commands; the subjects were instructed, however, to keep the hand relaxed. (6) The neuro-muscular set induced by the instructions to the subject was perhaps an important factor in determining the elicitation of the auto-commands. The subject was instructed to respond in a given manner at a given time. These instructions, therefore, served as stimuli for the organic set which resulted in the verbal stimuli being repeated by the subject at a given moment. The response of the pupil occurred only after the verbal stimulus was repeated. Therefore, we can say with some degree of certainty that the conditioned pupillary response appeared as an immediate result of the verbal stimulus, whether the latter was vocalized or subvocal. We can also assume that the set of the organism induced by the previous instructions plus associative processes were the important factors in evoking the verbal stimuli, and thus, indirectly, factors in determining the pupillary response. (7) How can one be certain that the subvocal (proprioceptive) stimuli were the important factors in arousing the pupillary responses and that this rôle was not played by afferent impulses generated elsewhere in the subject's body? I have said that incipient contractions of the hand may have been present, and elsewhere in the body muscular activities which had been conditioned to the verbal stimuli might be in progress. Such activities might be the important inciting factors rather than the subvocal verbal stimuli themselves. Such a possibility cannot be absolutely rejected; but the argument proves too much, for it is as applicable to the conditioned response to the bell as it is to the conditioned response to the subvocal stimuli. All that one can expect to have in any case is a correlation between the presence or absence of a stimulus and the presence and absence of a response. Thus, when the bell sounds, the pupil contracts; and when the bell does not sound, the pupil does not contract. In the absence of further positive data we are justified in concluding that the essential factor controlling the response is the bell. And the same argument is valid for the subvocal stimuli.

f. Experimental extinction. The conditioned responses both to the bell and to the hand movements were subject to experimental extinction. Re-enforcement was necessary at least once during any series of tests in order to secure the desired number of measurements. The response usually increased in size for the first 2 or 3 tests, gradually became weaker, and finally disappeared after from 4 to 8 tests. Ten trials of re-enforcement were sufficient to restore it to its former strength.

One of the interesting and important facts found is that no experimental extinction seemed to appear in the responses to the verbal stimuli. Tests were made at the end of the training periods, not only for the response to the verbal stimuli as given by the experimenter but also to these stimuli when self-excited by the subject. In no case during all of these measurements did the response disappear. The responses were larger, as a rule, than the responses to the bell or the hand movements, and they tended to continue as long as tests were made. Frequently as many as 25 to 30 tests were made at the end of the training period, with rest periods at intervals, but no re-enforcement was necessary.

The data on experimental extinction can best be presented in a table. In Table 3 I have given the results of the first series of tests for several subjects immediately following the training period. The tests are numbered in order and immediately beneath is shown the extent in millimeters of the conditioned pupillary constriction for each subject separately. The table shows that experimental extinction occurred from 3 to 8 consecutive tests when the bell was used as the conditioned stimulus. When the hand movement was used as the

TABLE 3

Experimental Extinction of Conditioned Responses during First Series of Tests after Training Series

Subject	No. of trials	Test numbers 1	2	3	4	5	6	7	8	9	10
Extinction of response to bell stimulus											
Tapn.	200	—.25	—.65	—.02	—.11	.00	—	—	—	—	—
Smt.	210	—.20	—.45	—.80	—.15	.00	—	—	—	—	—
Lght.	200	—.25	—.25	—.10	+.30	—	—	—	—	—	—
Hdgs.	200	—.10	—.20	—.35	—.05	.00	—	—	—	—	—
Hnt.	210	—.10	—.25	—.30	.00	—	—	—	—	—	—
Ktl.	250	—.40	—.10	—.50	—.40	—.80	—.70	—.75	.00	—	—
Vkp.	125	—.30	—.60	—.30	.00	+.50	—	—	—	—	—
Snpe.	150	—.75	—.95	—.45	—.75	—.45	—.50	—.20	.00	—	—
Extinction of response to hand movement stimulus											
Tapn.	200	—.20	—.55	—.25	.00	—	—	—	—	—	—
Smt.	260	—.60	—.50	—.60	+.20	.00	+.50	—	—	—	—
Lght.	240	—.10	—.30	—.15	+.05	+.10	—	—	—	—	—
Hdgs.	200	—.40	—.45	—.40	—.50	—.35	—.40	—.35	+.50	—	—
Hnt.	200	—.25	—.30	—.40	.00	—	—	—	—	—	—
Kmp.	190	—.50	—.50	—.60	—.60	.00	—	—	—	—	—
Vkp.	130	—.30	—.50	—.60	.00	+.20	—	—	—	—	—
Gce.	150	—.60	—.70	—.70	—.45	—.45	.00	—	—	—	—

Non-extinction to verbal command as stimulus

		Test numbers 1	2	3	....	19	20	21
Gce.	150	—.95	—.90	—1.40	....	—1.00	—1.20	—1.00
Smt.	200	—.50	—.30	— .60	....	— .60	— .80	—1.05
Hdgs.	200	—.70	—.70	— .65	....	— .90	— .60	— .75

conditioned stimulus experimental extinction appeared after from 3 to 7 tests. The non-extinction of the conditioned responses to the verbal stimuli is shown in the lower portion of the table. The results of the first three tests followed by the 19th, 20th, and 21st are given for three subjects. The results of the intervening 15 tests are omitted. No re-enforcement was necessary during any of these 21 tests. The response showed no signs of extinction. On the contrary, there is evidence of an increase in the extent of the constriction during the consecutive tests. The relative permanency of this response is further discussed in a later section (pp. 38-39). This absence of experimental extinction of the response to the verbal stimuli may be due to the fact that the subject had received more training at the time these latter tests were given than at the time when the tests for the effects of the bell and hand movements were made. After the tests for the response to the hand movements it was necessary to give from 100-200 trials before the responses to the commands alone were firmly established. In the absence of additional data any other explanation of the relative permanency of this response would have less basis in fact and would be mere speculation.

g. Latency and time of reaction of the conditioned response. The method of reading the diameter of the pupil before and after stimulation, while keeping the light constant, gives the dimensions of the changes, but it fails to give a complete picture of the response. It is impossible to obtain data on the latency and the duration of the response with such a method. In order to complete the picture, accurate records of the temporal course of the movement are needed. Photographic records would be the ideal method for this purpose, but since this method was not available at the time of the experiment some other method had to be used. The kymographic method was finally adopted. So far as the actual amount of change is concerned the method is as reliable as the method of reading the dimensions directly from the scale. In addition, the records give the path of the movement as it occurs in time, and from them it is possible to study the temporal relations of the responses. Errors in the tracings are of two sorts: (1) the reaction-time of the experimenter, and (2) over- or under-estimations of the actual movement. The errors of the first type are relatively constant, and those of the second type can be reduced to a minimum by practice.

The data from the kymographic records show that the conditioned pupillary reaction has a very long latency and contraction time. Much shorter in comparison are the latency and contraction or dilation times (as given by the authorities) for the light reflex and also for the "sensory reflex." The light reflex appears as a swift continuous contraction of the sphincter muscle which reaches a point of almost maximum contraction within 1 sec. The conditioned constriction, on the other hand, occurs at a much slower rate. Its progress is intermittent, characterized by spasmodic jerks, or steps, as the records show. The contraction time may be as great as 17 secs. The conditioned dilation which is established at the same time is of a similar type, but there is a reliable difference between the average dilation and the average constriction time (Table 5).

The contraction-dilation times of the two types of conditioned responses are independent of the actual extent of constriction or dilation. There is a wide variation in these values, as the large standard deviations of Table 5 indicate, but this variation is independent of the extent of the movement. A negligible correlation was found between the contraction or dilation time and the actual extent of the movement for both the conditioned dilation and the conditioned constriction response. The values of these correlation coefficients based upon 30 and 59 cases (Pearson's product moment) are .058, *P.E.* .08, and .201, *P.E.* .05, respectively.

There are four observable types of responses in the pupil, three of which were recorded. (The light reflex is too rapid to follow by my method.) (1) The sensory reflex, which is an unconditioned dilation response. This response appeared to all the stimuli before training began. (2) Conditioned dilation responses, characterized by longer latent and dilation times than the sensory reflex (Tables 4 and 5). (3) Conditioned constriction responses, characterized by even longer latency and constriction time than the conditioned dilation responses and more irregular in their course (Tables 4 and 5). The latent time, as used in this section, is defined as the time which elapses between the incidence of the stimulus and the beginning of the responses. The contraction or dilation time (time of reaction) is defined as the time which elapses between the incidence of the stimulus and the completion of the response. The contraction or dilation time thus includes the latent time.

Table 4 gives the average latent times with the number of cases and the measures of reliability for the three types of pupillary response mentioned above. The measurements were taken from the records of the two subjects for whom complete records were available.

The number of cases is too small to warrant final conclusions, but the data are indicative of what one may expect from more reliable data. The average latency of the sensory reflex is 1.01 secs., *S.D.* .43, while that for the conditioned dilation response is 1.56, *S.D.* .56 secs. The difference between the two means is .55 secs., its reliability is shown in the table. Table 4 also shows that the latency of the conditioned constriction is 2.29, *S.D.* 1.52 secs. The difference between this mean and the mean of the conditioned dilation response is also a reliable one. It is clear from these data that the conditioned constriction response has a longer latency than has that of conditioned dilation. A portion of the latency of the former response is taken up by the slight dilation of the pupil before it begins to constrict. A careful analysis of the available data reveals no significant differences between the latent times of the conditioned responses to the bell, the hand movement, and the verbal stimuli. This is also true of the time of reaction for these responses.

Table 5 gives the times of constriction and dilation for the same three responses. The average dilation time of the sensory reflex is

TABLE 4

AVERAGE LATENT TIME OF THE THREE TYPES OF PUPILLARY RESPONSE TO ALL STIMULI

Response	Latent time	σ	No.	Diff.	$\sigma_{diff.}$	$\frac{Diff.}{\sigma_{diff.}}$
Sensory reflex	1.01	.43	67			
Conditioned response dilation	1.56	.56	27	.55	.122	4.5
Conditioned response contraction	2.29	1.52	66	.73	.210	3.5

TABLE 5

AVERAGE RESPONSE TIME OF THE THREE TYPES OF PUPILLARY RESPONSES IN SECONDS

Response	Response time	σ	No.	Diff.	$\sigma_{diff.}$	$\frac{Diff.}{\sigma_{diff.}}$
Sensory reflex	5.36	1.32	67			
Conditioned response dilation	8.24	2.00	29	2.88	.40	7.2
Conditioned response contraction	10.93	2.75	74	2.68	.49	5.5

5.36, *S.D.* 1.32 secs. The dilation time for the conditioned dilation response is 8.24, *S.D.* 2.00 secs. The table shows the reliability of the difference between these two means. The contraction time for the conditioned constriction response is still longer, 10.93, *S.D.* 2.75 secs. The difference between the mean dilation time of the dilation response and the mean contraction time of the constriction response is also reliable.

The latency of the pupillary light reflex is given by Weiler (10, pp. 161-164) as .20 sec. Magitot (7, pp. 190-191) reports that Arlt and Vintschgau determined the latency of this reflex as .49 and .56 sec. respectively. The latency of the pupillary light reflex, if taken as lying somewhere between .2 and .5 sec., is much shorter than that of either of the conditioned responses which I have measured.

Weiler found that the contraction time for the light reflex is .87 sec. Reeves (8) gives no data for the latency of the light reflex, but does give data on the contraction and dilation times for the pupil when exposed to a bright light and to darkness. Reeves' curve, taken from the data of seven subjects, shows that the constriction of the pupil continues for 5 secs. when a strong light is directed into the eye. This curve, however, drops rapidly during the first 3-4 secs. The discrepancy between the values given for the contraction time of the light reflex reported by Reeves and Weiler may be due to the more exact methods of measurements used by the former author. These values, if inserted in Table 5, would rank the constriction time of the unconditioned light reflex as the shortest of the group.

Weiler (10, p. 180) also gives some data on the latency of the sensory reflex. He reports that the latency for the sensory reflex in response to electrical stimuli (pain) is .44 sec.; while the latency of the same reflex to a sudden noise is .36 sec. These values are much smaller than those presented for the same response in Table 4.

h. Permanency of the conditioned pupillary responses. No systematic attempt was made to test the permanency of the conditioned responses. Five of the subjects, however, returned for retests after intervals of from 15 to 90 days. Two subjects were tested 15 days after the final training period, and the conditioned responses to the verbal stimuli were still present without re-enforcement. Three other subjects returned for retests after intervals of 52, 60, and 90 days respectively. In two of these subjects (Ktl. and Hdgs., Table 6) the pupil responded by constricting when *either* of the words "contract" or "relax" were repeated by the experimenter. The range of these responses was from .20 to 1.00 mm. for the two subjects. In the case of subject Mkp. (Table 6, 90-day interval) only the sensory reflex remained. All of these subjects were given retraining following these tests. The results for the first retest and for the test after retraining are summarized in Table 6. The retraining for these subjects followed the procedure of Step III. Subject Hdgs. returned on the following day and received 50 additional training trials. She was tested each day for the following six days and at intervals during the following three months, but without further training. This subject was able, by subvocal excitation of the verbal stimuli, to constrict or dilate the pupil even when away from the experimental situation. No quantitative data were taken on the responses made while away from the experimental room, but the responses were large enough to be observable.

In the case of this subject, Hdgs., a new phenomenon appeared. While seated at the apparatus she could report correctly the direction of the pupillary changes which occurred, a capacity not possessed by the other 13 subjects. The usual reply of these 13 subjects to the question from the experimenter, "What did you do when I said 'contract'?" was, "I did nothing." The single subject referred to above was able to tell the experimenter the direction of the pupillary change in 100% of the cases. This fact appeared only after excess training. As an explanation of this phenomenon it would seem that the cue, or stimulus, by which the subject was able to report the pupillary changes was an indirect one. That is, since there are apparently no sensory nerve endings in the iris musculature which can be stimulated when the muscles contract, the cue must have come from some indirect source. The changing pupil causes a change in the actual amount of light which strikes the retina. It seems that in the case of this single subject these slight changes in illumination were supra-liminal and could be observed. Cason (1, p. 129) also reported that two of his subjects reported changes of illumination during the pupillary response.

i. Control experiments. I have already discussed (pp. 31-32), the various factors which may have determined the pupillary responses. There remains to be presented at this point a discussion of certain control experiments which were made and which also support the previously given conclusion that the verbal stimuli played the fundamental rôle in the "verbally conditioned" responses. These tests were as follows: (1) control for accommodation-convergence reac-

TABLE 6

Results of Retests with Verbal Stimuli Given by the Experimenter

Subject	Interval since last experimental period	Results of retest	No. of reenforced trials	Results of retest
Ktl.	52 days	constriction only	30	differential responses restored
Hdgs.	60 "	"	60	"
Mkp.	90 "	sensory reflex only	80	"

tions; (2) control for "meaning" of verbal stimuli; (3) reversal of the conditioned stimuli and the conditioning procedure; and (4) tests for the effects of the hand movements upon the process of conditioning.

1) *Accommodation-convergence reactions.* In all the experiments reported above an effort was made to keep accommodation and convergence constant by placing a fixation point (cross wires, *G*, Figure 1A) immediately above the light bulb, at a point 136 cms. from the eye of the subject, and by instructing the subject to fixate the point of intersection at all times while the light was on. (The subject was to keep the cross wires clear and distinct.) Even with this precaution, however, it was impossible to be certain that the instructions were being fully carried out. And it is known that an absolutely steady fixation is impossible. Since we know that pupillary changes accompany changes of accommodation and convergence, it is possible that conditioned pupillary responses were due to the fixation shifts rather than to the other stimuli presented. In order to test for this possible source of error it was necessary to find out the amount of change which occurred in the pupil when the subject shifted his accommodation and convergence throughout a considerable distance. Since the subject had been instructed to keep his fixation constant throughout the experiment, any shifts in fixation may be assumed to have been relatively slight and not discriminated by him, since he reported none. Even slight changes of fixation, however, may have some effect upon the pupil, and this type of effect must be determined before one can definitely rule out fixation shifts as a causal factor. For this purpose I required the subject to shift his fixation from one point to another over a distance of 116 cms. It may be assumed that the pupillary change caused by such

a shift of fixation would be greater than that due to any unintentional shifts of fixation which may have occurred during the experimental tests. The magnitude of the change due to such a large shift of convergence-accommodation can be compared with the magnitudes of change due to the conditioned stimuli. A second fixation point (cross wires) was so arranged that it could be brought within the subject's visual field directly in line with the distant fixation point and 20 cms. from the eye of the subject. With this arrangement it was possible to instruct the subject to accommodate for the distant fixation point, to measure the pupil, and then to introduce the nearer fixation point with the instructions to fixate it.

These tests were carried out, at the close of the work, on all but two subjects. Tests were made in both directions, near-distant and distant-near. The regular test light was used and from 4 to 6 readings were made for each subject. The average pupillary change observed in these tests was .31 mm., *S.D.* .08. The responses cover a range of .20 to .60 mm. Table 7 gives the comparison of the pupillary changes during these tests with the total average conditioned responses for the several stimuli. The total average *conditioned* changes are nearly three times as large as the *accommodation-convergence* pupillary change. This is conclusive evidence that within the limits measured (136-20 cms.) the pupillary changes which accompany accommodation-convergence reactions are not sufficient to account for the magnitude of the changes found in response to the conditioned stimuli. And surely any larger fixation movements would have been reported by the subjects.

2) *Nonsense words as verbal stimuli.* The results of most

TABLE 7

COMPARISON OF THE AVERAGE ACCOMMODATION—CONVERGENCE PUPILLARY CHANGE WITH THE TOTAL AVERAGE CONDITIONED CONSTRICTION

Conditioned stimuli	Total conditioned response	Accommodation convergence	Diff.
Bell	.88 mm.	.31 mm.	.57 mm.
Hand contraction	.87 mm.	.31 mm.	.56 mm.
Verbal stimuli "contract"	1.04 mm.	.31 mm.	.73 mm.
Auto- "contract"	1.18 mm.	.31 mm.	.87 mm.

of the experiments in which the verbal stimuli were used were obtained with the familiar words "contract" and "relax." The results in every case in which these words were used are clear. I have shown above that the conditioned responses were in all probability due to the verbal stimuli themselves rather than to extraneous factors. One factor, however, which was not discussed above and which may have been involved as a necessary factor in the control of the response was the factor of novelty or lack of novelty in the words used. It is possible that the words "contract" and "relax," involving, as they did, already well-established responses in the subject, aroused, by virtue of this fact, certain conditioned responses which themselves acted as stimuli for the pupillary response. This possibility, however, is not very probable since the initial effect of each of these familiar verbal stimuli was in every case a slight dilation of the pupil.

In order to secure factual material relative to this hypothesis controls were made with two subjects using nonsense words. One untrained subject was conditioned by the usual procedure, the only change being the substitution of "torpo" and "somet" for "contract" and "relax." It was found that when the nonsense words were used to elicit the hand movements they became in the end conditioned stimuli for the pupillary responses. Tables 1 and 2, subject Vkp., give a summary of the results for this subject. The second subject, also previously untrained, received slightly different training. (See Control 4.) The final results, however, indicate that the nonsense syllables "sig" and "tok" can become adequate conditioning stimuli for the conditioned pupillary responses. Further evidence regarding extraneous factors involved in the novelty or lack of novelty of the word stimuli is furnished in connection with Controls 3 and 4.

3) *Reversal of the conditioned sitmuli.* With most of the subjects the bell stimulus, the hand contraction, and the verbal stimulus "contract" were used as conditioned stimuli for pupillary constriction, while the hand relaxation and the verbal stimulus "relax" were used as conditioned stimuli for the dilation response. In order to demonstrate that the conditioned responses thus established were due to the conditioning method rather than to aspects of the stimuli arising from conditions effective prior to the beginning of experimentation, one subject was trained with these stimuli reversed. In Step I the bell was associated with darkness. Thus the bell rang

while the pupil was dilating. In Steps II and III the stimuli were similarly reversed. The dynamometer switch was arranged so that both the bell and light were on while the hand was relaxed. When the hand contracted at the verbal stimulus "contract" both light and bell circuits were broken. In all other respects the experiment was conducted in the same manner as were the others. The data for this experiment were recorded only for the conditioned dilation responses. The average difference between the pupillary response to the various stimuli prior to the training and the response after training for this subject (Jffe., Table 1) may be summarized as follows: bell, .57 mm.; hand contraction, .23 mm.; verbal stimulus from the experimenter, .55 mm.; and "auto-contract," .53 mm. No test was made for the subvocal stimuli.

4) *Tests for the effect of the hand responses.* It seemed worthwhile to plan an experiment which would demonstrate any vital rôle played by the hand responses of the subject during the conditioning procedure. Early preliminary experiments had shown that it was difficult to establish verbal stimuli as conditioned stimuli for pupillary responses when associated with the light alone. The dynamometer switch and the hand movements were introduced in order to overcome that difficulty. In order to test the possibility of establishing verbal stimuli as conditioned stimuli without the intermediate step of using the hand movements the following experiment was performed: One subject (Snpe., Tables 1 and 2) was used. The pupil was first conditioned to the bell stimulus in the usual manner. One hundred and forty trials were sufficient for this. Following the conditioning to the bell the subject was given 100 trials each day for 4 days with the nonsense syllables "sig" and "tok" paired with light and darkness respectively. For the first 200 trials the bell was used along with the light much the same as in the usual procedure of Step II, except that the hand movement was not used. For the second 200 trials the bell was omitted. The procedure was as follows: The experimenter repeated "sig" and immediately turned on the light (or light and bell). Four seconds later he repeated "tok" and broke the light circuit. Tests were given in the usual manner at the end of each training period. *At the end of the 4th day (410 trials), tests showed negative results, no conditioning had resulted.* On the following day the hand movements were introduced. Tests were made at the beginning of this period for the initial effect of the hand responses. The usual slight

dilation followed both the hand contraction and the hand relaxation. *At the end of only 80 trials of training with the hand responses and the nonsense syllables, tests showed that the pupil had become conditioned to these stimuli.* Further tests showed that the nonsense syllables alone now evoked the constriction and dilation responses of the pupil. The subject could repeat the syllable himself and so elicit the responses, which were quite as large as any of those obtained from any of the other subjects in the previously described experiments.

Since only one subject was used in this experiment, no final conclusion can be drawn. There are clear indications, however, that the hand responses, which were used to release the unconditioned stimuli and which were in turn elicited by the verbal stimuli, have a significant rôle in the process by which the verbal stimuli become conditioned stimuli for this pupillary response. Further experimentation is in progress in an effort to clarify this point.

Interpretative Comments

I suggested in the introduction to this paper that positive results from the proposed experiment should give the basis for a better understanding of that form of behavior known as voluntary. The literature on voluntary activity, both theoretical and experimental, fails to give volition any definite formulation that would make it a useful concept in experimental science. The will is defined as one of the aspects of mental life, the aspect of foresight, purposive striving, and decision. Voluntary behavior is thus the result of this psychic activity, and it is to be distinguished from other forms of learned behavior by reason of its mental antecedents and determinants. Experiments have been made with some success on the problem of the acquisition and control of "voluntary" behavior; but no criteria have been given by means of which such behavior can be distinguished from "non-voluntary" responses. Such efforts as have been made to discover by introspection the actual mental factors which initiate and control behavior have resulted in much confusion. Sensory images of the movement itself, either resident or remote, have been proposed by some investigators. These factors have been rejected by others in favor of feelings of innervation, imageless thoughts, and previsions of the end of the movement.

Too much emphasis cannot be laid upon the fact that if a given phenomenon is to be studied it must be identifiable. When various forms of behavior are observed, by what criteria are we to identify some as voluntary and others as non-voluntary? Perhaps, after all, these concepts are not scientifically useful. Perhaps we can make better experimental progress if we discard them and seek to classify behavior on other grounds.

There are three important factual results of the present study. In the first place, the data presented confirm the findings of Cason that smooth muscle responses can be conditioned to the sound of a bell. In addition, the data indicate that it is possible to condition such smooth muscle activity to motor responses made by the subject himself, even though the smooth muscle is unprovided with afferent nerve endings. The motor responses involved may be hand contractions and relaxations or verbal responses, either vocal or subvocal. The conditioned response may thus be elicited, independently of any auditory component of the verbal stimuli, by the organism itself through the use of stimuli which, while not directly observable by the experimenter, are nevertheless inferable from the data of the training and control experiments. In the absence of any evidence to the contrary, we may assume that the appearance of self-induced stimuli is under the control of the associative mechanism and that this appearance involves the activity of already established conditioned responses. The heavy burden of proof lies upon him who would defend the hypothesis that the vocal verbal stimuli are aroused in the subject through acts of *will.*

In the second place, the experimental and control data indicate that it is possible to set up conditioned responses in which the factor of experimental extinction is apparently inoperative. *Although the results upon the non-experimental extinction of the verbally conditioned response need further verification, too much emphasis can hardly be place on the present findings.* Many writers have attacked the theory that conditioned responses constitute the basis of all learning on the ground that the factor of experimental extinction precludes such a theory. The present findings may well be an opening wedge in the overthrow of that criticism.

In the third place, the results of the present experiment confirm the findings of others regarding the relative complexity of the conditioned response as compared with the unconditioned response of the same organ. Hilgard (3, pp. 37-38), 1931, for instance, says: "Any diagrammatic simplification which makes the conditioned response simply the old unconditioned reflex attached to a new stimulus does violence to a process which involves numerous modifying factors. . . . there is little justification for referring to conditioning as stimulus substitution. The result is alteration and addition rather than substitution." The data presented in this paper confirm Hilgard's position.

Recent investigations of conditioning in human subjects have revealed a very close parallel between conditioned and voluntary activities obtained from the same effector group, when the quantitative aspects of the two forms of behavior are compared. Thus Hamel (2) and Humphrey (4) found that the latency of the conditioned finger withdrawal and that of voluntary discrimination responses of the same organ were identical. Schlosberg (9) and Wendt (11) found similar relationships between the conditioned and the voluntary knee-jerk. In addition, these two authors report that the forms of the two responses were identical. Hilgard (3) reported identical latencies for the conditioned lid-reflex and the voluntary wink, and he also found a close similarity in the forms of the two responses. Each of the above investigators reported that the latency and form of response for the original reflexes were markedly different from the same aspects of the conditioned and voluntary responses. It is clear, therefore, that while "voluntary" activity is different from the corresponding reflex, it is not to be distinguished from the corresponding conditioned response on the basis of the temporal characteristics of the behavior.

The authors mentioned above do not tell us what voluntary activity is. When the subject was instructed to respond in a given manner at a given time and did so, the response was called voluntary; but when the response was established by the method of paired stimuli, and when it was then elicited by some stimulus external to the organism, the activity was called a conditioned response. No differences were observed in the two responses by the experimenter (without instrumentation), but the subjects were able to report a "prevision" of the voluntary response before it appeared, a not-unexpected result because of the preceding verbal instructions. It is also worth noting that in the case of the so-called voluntary activity no data on the genesis of the response was at hand and that the behavior was apparently controlled by intra-organic conditions.

In my own study the vocal verbal and the subvocal verbal conditioned pupillary responses look as much like voluntary responses as do the so-called voluntary knee-jerks and lid-reflexes. However, in the case of the pupillary responses we have the history of their genesis, a history which is that of conditioning. Foresight, or prevision of the end, was eliminated by working with naïve subjects and by utilizing a response mechanism unsupplied with afferent nerve endings. Had these two factors not been eliminated the explanation of the resulting behavior would have been more complicated than it is but not different in principle.

In conclusion, I shall quote a passage from Hunter and Hudgins (6): ". . . we are of the opinion that the attempted classification of behavior into voluntary and non-voluntary forms of response is of less significance than a classification of the corresponding behavior in terms of genesis, type of control, and temporal characteristics. An adequate account of what the psychologist has called voluntary action cannot as yet be given because the necessary experimental data have not yet been secured; but the hypothesis is offered that so-called voluntary behavior is essentially a conditioned response having a characteristic latency and temporal course and under the control of self-excited receptor processes."

SUMMARY

1. The problem investigated was that of conditioning the pupil of the human eye to verbal stimuli and to other stimuli under the subject's control. It was thought that positive results from such an experiment would lead to a better understanding of so-called voluntary activity.

2. The conditioning procedure was carried out as follows:

a. The pupil was first conditioned to a bell with the light as the unconditioned stimulus.

b. A hand contraction on the part of the subject was next used to close the light and bell circuits. The relaxation of the hand broke the same circuits. Verbal commands from the experimenter were used to evoke the hand responses.

c. The bell and the hand responses were finally eliminated leaving only the verbal commands as conditioned stimuli.

d. Finally, the subjects were required to repeat the verbal stimuli vocally, then by whispering, and finally subvocally.

3. The results from 14 subjects indicate that verbal stimuli can become conditioned stimuli for the pupillary response. Conditioned pupillary responses (both dilation and constriction) can be evoked by verbal stimuli from the experimenter or by verbal stimuli self-presented by the subject either vocally, whispered, or subvocally.

4. Of the 14 subjects 10 were naïve and did not know that their pupillary reactions were being conditioned. It is therefore impossible that their verbally controlled reactions should have been influenced by purposes or intentions. Furthermore, inasmuch as there are no afferent nerve endings in the iris, the conditioned pupillary responses could not have been controlled by nervous impulses from the responding organ. In spite of these two facts, the verbally controlled conditioned pupillary responses had that appearance of spontaneity and control by the organism which are so characteristic of the behavior called voluntary.

5. Control experiments indicate: (1) that the pupillary responses described could not be due to changes of accommodation and convergence; (2) that nonsense words and syllables could be substituted for the familiar "contract" and "relax" without affecting the final result; (3) that the conditioned dilation and constriction responses could be established regardless of the conditioned stimulus (contract or relax) which was paired with the unconditioned stimulus; and (4) that the hand responses on the part of the subject were essential in establishing the verbal stimuli as conditioning stimuli.

6. The conditioned pupillary responses to the verbal stimuli were relatively permanent. No experimental extinction occurred during any of the tests with the verbal stimuli and evidence is given which indicates that after an interval of 15 days the responses were still present. The conditioned responses to the bell and to the hand responses were subject to the usual experimental extinction.

7. The temporal aspects of the conditioned pupillary response differ from those of the unconditioned light reflex in the following manner:

a. The latency of the conditionel response is from 5 to 10 times larger than the latency of the unconditioned light reflex.

b. The duration of the conditioned response is from 3 to 5 times larger than that of the light reflex.

8. "An adequate account of what the psychologist has called voluntary action cannot as yet be given because the necessary experimental data have not yet been secured; but the hypothesis is offered that so-called voluntary behavior is essentially a conditioned response having a characteristic latency and temporal course and under the control of self-excited receptor processes."

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LE CONDITIONNEMENT ET LE GOUVERNEMENT VOLONTAIRE DU RÈFLEXE PUPILLAIRE DE LUMIÈRE

(Résumé)

1. Le problème étudié a été celui de conditionner la pupille de l'oeil humain aux stimuli verbaux et à d'autres stimuli sujets au gouvernement du sujet. On a cru que des résultats positifs d'une telle expérience nous donneraient une meilleure compréhension de la soi-disant activité volontaire.

2. Le processus de conditionner a été le suivant: (*a*) La pupille a été d'abord conditionnée à une sonnette, la lumière étant le stimulus non conditionné. (*b*) On a ensuite employé une contraction de la main du sujet pour établir les circuits de la lumière et de la sonnette. La relaxation de la main a coupé les mêmes circuits. On a employé des ordres verbaux de l'expérimentateur pour évoquer les réponse de la main. (*c*) Les réponses de la sonnette et de la main ont été enfin éliminées, laissant seulement les ordres verbaux comme stimuli conditionnés. (*d*) Enfin, on a fait répéter les stimuli verbaux par les sujets à haute voix, ensuite en chuchotant, et enfin silencieusement.

3. Les résultats de 14 sujets indiquent que les stimuli verbaux peuvent devenir des stimuli conditionnés pour la réponse pupillaire. Des réponses pupillaires conditionnées (dilation et contraction) peuvent être évoquées par des stimuli verbaux de l'expérimentateur ou par des stimuli verbaux présentés par le sujet lui-même à haute voix, en chuchotant, ou silencieusement.

4. Dix des 14 sujets ont été naïfs et n'ont pas su qu'on conditionnait leurs réactions pupillaires. Il est donc impossible que leurs réactions gouvernées verbalement aient été influencées par des buts ou des intentions. D'ailleurs, puisqu'il n'y a pas de bouts de nerfs afférents dans l'iris, les réponses pupillaires conditionnées n'ont pu être gouvernées par des impulsions nerveuses de l'organe qui répond. Malgré ces deux faits, les réponses pupillaires gouvernées verbalement ont eu cette apparence de spontanéité et de gouvernement par l'organisme lesquels sont si caractéristiques du comportement appelé volontaire.

5. Des expériences de contrôle indiquent: (1) que les réponses pupillaires décrites n'ont pu être dues aux changements de l'accommodation et de la convergence; (2) que des mots et des syllabes non-sens ont pu remplacer les mots bien connus "contractez" et "relâchez" sans influer sur le résultat final; (3) que les réponses conditionnées de dilatation et de contraction ont pu s'établir sans égard pour le stimulus conditionné (contractez ou relâchez) couplé avec le stimulus non conditionné; et (4) que les réponses de la main par le sujet ont été essentielles pour établir les stimuli verbaux comme stimuli conditionnants.

6. Les réponses pupillaires conditionnées aux stimuli verbaux ont été relativement permanentes. Nulle extinction expérimentale ne s'est montrée pendant aucun test avec les stimuli verbaux et on présente de l'évidence qui indique qu'après un intervalle de 15 jours les réponses ont été toujours présentes. Les réponses conditionnées aux réponses de la sonnette et de la main ont été sujettes à l'extinction expérimentale usuelle.

7. Les aspects du temps de la réponse pupillaire conditionnée diffèrent de ceux du réflexe de lumière non conditionné de la manière suivante: (*a*) La latence de la réponse conditionnée est plus grande de 5 à 10 fois que la latence du réflexe de lumière non conditionné. (*b*) La durée de la réponse conditionnée est plus grande de 3 à 5 fois que celle du réflexe de lumière.

8. "On ne peut pas encore décrire d'une façon satisfaisante ce que le psychologue a appelé l'action volontaire parce que l'on n'a pas encore obtenu les données expérimentales nécessaires; mais on offre l'hypothèse que le comportement soi-disant volontaire est essentiellement une réponse conditioneé qui a une latence et une durée caractéristiques et qui est gouvernée par des processus excités par le récepteur lui-même."

Hudgins

DIE BEDINGUNG UND WILLKÜRLICHE BEHERRSCHUNG DES LICHTREFLEXES DER PUPILLE

(Referat)

1. Man versuchte in dieser Untersuchung die Pupille des menschlichen Auges zu einer Reagierung auf Wortreize und auf andere unter der Herrschaft der Versuchsperson liegenden Reize zu bedingen (condition). Man dachte, positive Befunde aus einer solchen Untersuchung würden zu einem besseren Verständnis der sogenannten willkürlichen Tätigkeit (voluntary activity) führen.

2. Die Bedingung wurde folgenderweise ausgeführt: (*a*) die Pupille wurde zuerst auf eine Glocke bedingt; bei dieser Bedingung diente das Licht als unbedingter Reiz. (*b*) Dann wurde eine Zusammenziehung der Hand durch vie Versuchsperson zur Schliessung der Stromkreise des Lichtes und der Klingel verwendet. Die selben Stromkreise wurden durch Erschlaffung der Hand gebrochen. Zur Hervorrufung der Handreaktionen dienten Wortbefehle des Versuchsleiters. (*c*) Schliesslich wurden die Glocke und die Handreaktionen ausgeschaltet, und die Wortbefehle allein als bedingte Reiz hinterlassen. (*d*) Schliesslich wurden die Versuchspersonen ersucht, die Wortreize zuerst laut, dann flüsternd, und schliesslich still für sich (subvocally) zu wiederholen.

3. Die an 14 Versuchspersonen ermittelten Befunde weisen darauf hin, dass Wortreize bedingte Reize zur Reaktion der Pupille werden können. Bedingte Reaktionen der Pupille (sowohl Ausdehnung wie Zusammenziehung) können durch Wortreize von Seite des Versuchsleiters oder durch Wortreize die sich die Versuchsperson selbst, laut, flüsternd, oder still gibt, hervorgerufen werden.

4. 10 der 14 Versuchspersonen waren naif und wussten nicht, dass man ihre Pupillenreaktionen bedingte. Ihre verbal kontrollierten Reaktionen hätten also unmöglich durch Absichte oder Zwecke beeinflusst worden sein können. Ferner, da sich in der Regenbogenhaut keine afferenten Nervenendungen finden lassen, hätten die bedingten Pupillenreaktionen unmöglich durch Nervenimpulse aus dem reagierenden Organ bedingt worden sein können. Trotz dieser zwei Tatsachen hatten die verbal kontrollierten bedingten Reaktionen der Pupille jenes Aussehen der Ungezwungenheit und der Kontrollierung durch den Organismus die für die sogennante willkürliche Tätigkeit (voluntary behavior) so karakteristisch ist.

5. Kontrollversuche weisen darauf hin; (1) dass die beschriebenen Pupillenreaktionen nicht durch Änderungen der Anpassung und der Konvergenz bedingt sein können; (2) dass die wohlbekannten Befehlwörter "Zusammenziehen!" und "Ausdehnen!" ohne Beinflussung der endgültigen Befunde durch sinnlose Wörter oder Silben ersetzt werden können; (3) dass die bedingten Reaktionen der Ausdehnung und des Zusammenziehens gegründet werden konnten, dessen unbeachtet, ob der bedingte Reiz, der mit dem unbedingten Reiz gepaart wurde, "Zusammenziehen!" oder "Ausdehnen!" war; und (4) dass die Handreaktionen seitens der Versuchsperson bei der Entwickelung der Wortreize in bedingende Reize wesentlich waren.

6. Die bedingten Pupillenreaktionen auf die Wortreize waren relativ standhaft. Es fand niemals im Laufe der Versuche mit den Wortreizen experimentelles Erlöschen (extinction) statt, und es weisen Befunde darauf hin, dass nach Verlauf von 15 Tagen die Reaktionen noch immer da waren. Die bedingten Reaktionen auf die Glocke und auf die Handreaktionen waren der gewöhnlichen experimentellen Erlöschung unterworfen.

7. Vom zeitlichen Standpunkt aus ist die bedingte Pupillenreaktion von dem unbedingten Lichtreflex folgenderweise verschieden: (*a*) die Latenzzeit (latency) der bedingten Reaktion ist 5 bis 10 Mal so lang wie die des unbedingten Lichtreflexes. (*b*) Die Dauer der bedingten Reaktion ist 3 bis 5 Mal länger, als die des Lichtreflexes.

8. "Das, was der Psycholog willkürliche Tätigkeit nennt, kann heute noch nicht zufriedenstellend beschrieben werden, weil die notwendigen experimentellen Befunde noch fehlen; es wird aber die Hypothese angeboten, dass die so-gennante willkürliche Tätigkeit wesentlich eine bedingte Reaktion ist, die eine karakteristische Latenzzeit hat und unter der Herrschaft selbst-erregter Rezeptorvorgänge (self-excited receptor processes) steht."

HUDGINS

DOES POSITIVE RESPONSE TO DIRECT SUGGESTION AS SUCH EVOKE A GENERALIZED HYPERSUGGESTIBILITY*

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Clark L. Hull, Everett F. Patten, and St. Clair A. Switzer

One of the most puzzling questions in modern psychology is that concerning the nature of hynopsis. Bramwell's (2) lengthy review of hypnotic theories attests the serious attention which has been devoted to the subject. A perusal of that array of divergent opinion emphasizes once more the desirability of scientific hypotheses being sufficiently concrete and tangible that:

1. Definite deductions of specific phenomena and relationships hitherto unobserved may be made from them.
2. Phenomena and relationships thus deduced may be susceptible of concrete experimental test.

Were this practice as general as the recognition of its desirability there would be less confusion than at present, not only in the field of hypnosis but in the science of psychology in general. The present article describes an investigation which attempts to proceed in conformity with the above principles.

THE HYPOTHESIS

It has been shown repeatedly that, whatever else hypnosis may be, it is a state of heightened susceptibility to suggestion (3, 5, 10). Moreover, despite the long-standing belief to the contrary, it is doubtful whether any phenomena take place in the hypnotic trance which may not be observed in lesser degree under otherwise similar circumstances in the non-trance state. These considerations naturally focus attention upon the phenomenon of hypersuggestibility. They give special significance to the question of why a state of generalized hypersuggestibility should be produced by the suggestions which lead to a closure of the lids and a naïve simulation (1) of sleep. Is this increased susceptibility to suggestion a result peculiar to the particular

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suggestions given to induce hypnosis, or is it a generalized characteristic of all successful direct suggestion to movement?

Certain known experimental phenomena of hypnosis appear to conform to the latter view. In 1929, Hull and Huse (5) found that the second reaction of two postural suggestions required about a fourth less time than the first, regardless of whether the two suggestions were given in the trance or the waking state. Clearly, the positive response to the first suggestion in some literal sense increased the susceptibility to the second. In this connection they remark:

> "Presumably the principle is very general. Having responded to one suggestion, the organism is at once in a state of heightened susceptibility to further suggestion."

According to this hypothesis, then, the positive response to the direct suggestion to fall into a postural state resembling sleep physiologically generates a heightened susceptibility to all kinds of other suggestions, including that to sway forward in the postural suggestion experiment.

It happens, however, that there is a well-known process not ordinarily associated with suggestion which also manifests an increased facility at the second of two repetitions—the familiar results of practice which produces habituation. In this connection it is important to recall that Hull and Huse (5), Williams (10), and especially Patten, Switzer, and Hull (9), have shown that ordinary waking suggestion manifests extremely typical practice phenomena. Krueger (7) has shown the same for hypnosis. These considerations precipitate the critical question of whether hypersuggestibility can be differentiated as an entity distinct from practice effects.

A clue to the possible differentiation of the two supposed phenomena may lie in the fact that hypersuggestibility is usually thought of as a distinctly transitory phenomenon, whereas practice effects result in more or less permanent facilitation. Krueger (7) seems to have shown, for example, that the hypersuggestibility characteristic of hypnosis, as manifested by increased susceptibility to the trance itself, is largely lost within a period of two hours after dehypnotization. At this point a complication in the experimental differentiation of the supposed entities enters in the fact that certain forms of learning where practice periods are separated by as much as 24 hours also show very large losses of each practice facilitation during such an interval. However, if a suggestion should be repeated until the subject has reached the limit of practice, i.e., until successive trials no longer show any mean gain in speed of reaction lasting over a period of 24 hours, there ought still to be a gain during each period of practice if hypersuggestibility is a distinct entity. Patten, Switzer, and Hull attempted to isolate hypersuggestibility according to this principle by repeatedly securing positive responses to waking postural suggestion. The experiment proved inconclusive because the subjects at the end of the 20 days of practice employed had not yet reached the limit of permanent gain. There was an indication, however, that with this form of suggestion very little or no hypersuggestibility exists.

The approach of Patten, Switzer, and Hull was from the point of view of the effects of practice upon the activity being exercised. The most convincing evidence of hypersuggestibility so far obtained experimentally (3, 5, 10) falls, on the other hand, in the category of transfer of training. The transfer is from the activity of going into the trance to the distinct activity of responding to some postural suggestion not particularly associated with hypnosis. Now if this principle were general, it is clear that the above relationship should be reversible—response to postural suggestions in the waking state should also facilitate the rate of lid closure upon falling into the trance. This latter deduction was submitted to experimental trial, as an incident to another investigation by Caster and Baker (3). While the conditions of their experiment were not such as to lead to the expectation of a large effect, these authors report results slightly favorable to the hypothesis. Informal reports from a second investigation involving substantially the same variables in still another laboratory appear also to favor the hypothesis.

The Deduction

If hypnosis is nothing more than a special case of a tendency for all responses to direct suggestion to generate a heightened susceptibility not only to the same act but to all kinds of other acts, then the principle should be operative reciprocally between two distinct types of purely waking suggestion. Concretely, if subjects have responded positively to a suggestion that the head shall fall forward upon the chest, immediately after they should respond with a heightened facility to the suggestion to sway forward the horizontally extended arm; if subjects have responded positively to the suggestion to sway forward the horizontally extended arm, immediately after they should show a heightened susceptibility to suggestion that the head shall fall forward upon the chest. Here, then, we find the specific problem of the present investigation.

SUBJECTS, PROCEDURE, ETC.

Thirteen naïve male subjects, mostly college freshmen in their second semester, were selected from a group of 40 volunteers by means of a preliminary test of waking responsiveness to direct suggestion. The criterion of choice was that the blindfolded subject should sway forward to the extent of losing balance within 3 minutes, in response to suggestions of falling forward delivered by a phonograph.

The individuals finally selected reported to the psychological laboratory at the same hour on 15 consecutive days. On each occasion the subject was given two suggestions to head movement and two to arm movement. After the subject had been blindfolded and placed in the apparatus, one minute was allowed before suggestions began, and each of the three succeeding suggestions were begun one minute after the termination of the preceding one. No words were spoken to the subject during the adjustment of the apparatus except irrelevant casual conservation. After that, the only words heard by the subject were the suggestions delivered by a modern Victor phonograph with electrical reproduction from specially prepared disks. This method of stimulation was resorted to in order to insure absolutely objective uniformity in the stimulations, and, in particular, to protect the results from the danger that the expectation of a certain outcome on the part of the experimenter might unconsciously influence his voice while giving the suggestions, in such a way as to bring about its realization (6). In order to eliminate constant errors from practice effects, on half of the experimental occasions the two head suggestions preceded, and on the remainder the two arm suggestions preceded. So far as possible with an odd number of days, the *A B B A* method of alternating the two sequences of suggestion was followed. As an added circumstance providing further protection against constant error from practice effects, the order of the head suggestion was necessarily opposite that for the arm throughout the 15 days. The program of experimentation is shown in detail by Table 1.

The measure of susceptibility to head suggestions was the time required to make a forward movement of 6 inches (measured from the top of the head) with 11 subjects, and 4 inches each on 2 others who happened to have short necks. The measure of susceptibility to arm suggestions was the time required for the arm to move a point 30 inches from the shoulder, a distance of 8 inches with 5 subjects and of 6 inches with 8 subjects. With a given subject the extent of a given movement employed was constant throughout the experiment, so that strict comparability of the two conditions under investigation was maintained. All movements were traced on the smoked paper of an extension kymograph by means of Hull recorders (4, p. 154). The movements of the head were transmitted to one of the two recorders by a thread lightly hooked to a heavy pad attached to the blindfold, the former running over the top of the head from front to back.

Because of the necessity of protecting the extended arm from fatigue, a special piece of apparatus was developed to support it.[1] This consisted of a light wooden platform shaped like a segment cut from a 60-inch circle. This was suspended by a line from a pulley attached to the ceiling of the room. By varying the length of the suspended line, the height of the platform could be adjusted exactly to fit the sitting heights of the various subjects. The truncated point of the platform was placed beneath the arm just free from the vertical portions of the subject's garments. The curved portion of the platform fell at a point a little beyond the ends of the subject's fingers and 30 inches from the shoulder joint. To this arc was attached a properly curved plate of sheet metal, 3 inches high. The thread communicating with the recording instrument was attached to the front edge of this plate. As the arm swung the supporting platform forward, the thread wrapped around the curved sheet metal as it might over the curved surface of a drum.

Approximately half of the subjects were put through the experiment by Patten, and the remainder by Switzer. Since observation has seemed to show more effective results when phonographic suggestions are given in the reproduced voice of the person conducting the experiment, substantially duplicate disk records were made up for each experimenter to be used by him exclusively. Separate 3-minute records were made for the head and for the arm. The transcription of the early portion of Switzer's head-suggestion record is as follows:

> "Now for just a few minutes please try to forget everything except the words you hear. Think only of what I say. Think of your head falling forward on your chest. Your head is falling forward on your chest. Head is falling forward. Your head is falling forward. Head is falling forward on your chest. Head is falling forward. Head is falling forward.

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[1] A perspective drawing of this apparatus showing subject in position is published by Caster and Baker (3).

TABLE 1

The Detailed Suggestion Scores of a Typical Subject

The entries are the number of seconds required for constant amounts of extended-arm and head movements under the influence of continuous verbal suggestion delivered by a phonograph. (*H*)=head, (*A*)=arm.

Day of practice														
1	2	3	4	4	6	7	8	9	10	11	12	13	14	15
(H) 82.4	(A) 39.6	(A) 25.8	(H) 44.5	(H) 56.0	(A) 27.8	(H) 44.1	(A) 28.0	(A) 20.7	(H) 54.4	(A) 35.1	(H) 55.1	(H) 51.7	(A) 24.1	(H) 55.3
(H) 78.6	(A) 22.8	(A) 21.6	(H) 44.1	(H) 35.5	(A) 16.2	(H) 49.4	(A) 22.0	(A) 19.4	(H) 75.0	(A) 32.8	(H) 53.4	(H) 49.5	(A) 19.0	(H) 46.0
(A) 57.1	(H) 54.5	(H) 60.0	(A) 21.6	(A) 16.6	(H) 51.5	(A) 20.7	(H) 44.3	(H) 38.2	(A) 44.4	(H) 76.9	(A) 26.8	(A) 29.5	(H) 45.5	(A) 27.1
(A) 30.0	(H) 60.4	(H) 42.4	(A) 20.6	(A) 25.6	(H) 38.1	(A) 17.5	(H) 37.2	(H) 33.2	(A) 32.2	(H) 67.5	(A) 24.5	(A) 25.0	(H) 44.1	(A) 25.6

> Farther forward, farther forward. Your head is falling farther forward. Falling farther forward. Farther forward, farther forward. Your head is falling farther forward. Head falling forward on your chest. Falling farther forward on your chest. Head is falling farther forward. Head is falling farther forward. Farther, farther, farther still, farther still, farther still, farther still, farther forward, farther forward. Head is falling farther forward. Head is falling farther forward. Farther forward. Falling forward on your chest, etc."

The transcription of the corresponding portion of Switzer's arm-suggestion record is:

> "Now for just a few minutes please try to forget everything except the words you hear. Think only of what I moving to the left. Your arm is moving to the left. Your arm is swinging to the left. Your arm is moving, moving, moving to the left. Your arm is moving to the left. Your arm is moving to the left, arm is moving, moving, moving to the left. Your right arm is moving to the left. Your right arm is swinging farther, farther, farther, farther still, farther still, farther to the left, farther to the left. Your right arm is moving farther to the left, farther still, farther still. Think of your arm moving to the left, moving to the left. Think, think of your arm, moving, moving, moving to the left. Your right arm is moving to the left. Your right arm is moving to the left. Swinging, swinging, swinging farther, swinging farther, swinging farther, swinging farther, farther still, farther still, farther to the left, farther to the left. Your arm is moving farther, farther, farther to the left, farther still, farther still, etc."

RESULTS

The detailed scores of a typical subject are shown in Table 1. The results of the entire 780 suggestion measurements from all 13 subjects are summarized systematically from the point of view of the present problem in Table 2. While the subjects differ somewhat among themselves, the general picture is clear. The hypothesis that positive response to direct suggestion as such evokes a generalized hypersuggestibility is definitely negated. The mean suggestion time for the first arm movement when following two head-movement responses, instead of being smaller than when preceding the head movements (as demanded by hypothesis), is actually larger. The former is 59.8 sec., the latter 56.2 sec. That a genuine reverse relationship exists is unlikely, however, since the critical ratio of this difference

TABLE 2

SUMMARY OF MEAN NUMBER OF SECONDS REQUIRED FOR CONSTANT AMOUNTS OF EXTENDED-ARM AND HEAD MOVEMENT UNDER THE INFLUENCE OF CONTINUOUS VERBAL SUGGESTION

Subject	Arm movement				Head movement			
	Preceding head movement		Following head movement		Preceding arm movement		Following arm movement	
	1st trial	2nd trial	1st trial	2nd trial	1st trial	2nd trial	1st trial	2nd trial
Ro	28.7	22.0	30.5	25.1	55.4	53.9	53.0	46.1
Cl	97.6	97.0	107.8	91.7	81.3	66.2	108.2	90.2
Te	58.2	58.4	83.5	61.9	100.1	72.8	106.2	70.7
O'B	52.7	42.8	41.0	56.1	59.4	52.2	53.2	45.2
Br	66.5	69.6	63.3	56.7	92.5	78.6	81.0	79.0
Lo	44.6	42.0	52.2	45.9	72.8	73 2	79.3	75.1
Pa	57.4	41.4	44.1	32.7	59.5	39.2	54.0	42.4
Lor	68.9	53.2	80.7	68.3	57.1	40.7	50.6	43.0
Bu	93.4	84.2	97.9	82.8	87.8	79.4	95.0	84.1
Bo	51.1	41.3	56.9	47.3	54.0	47.7	48.9	42.8
Rh	35.3	31.3	37.1	32.6	72.0	55.5	69.7	52.6
Ba	45.1	43.7	49.6	45.9	54.2	42.9	45.8	44.9
Mo	30.8	27.0	32.6	32.1	40.9	37.6	33.4	32.1
Mean	56.2	50.3	59.8	52.2	68.2	56.9	67.6	57.6
$P.E._m$	3.85	3.95	4.54	3.62	3.19	2.76	4.37	3.48

to its probable error is only 1.73 ($r=.89$). The same negative relationship obtains with the pair of means of second arm trials, though again a critical ratio of 1.31 ($r=.93$) does not indicate a significant difference. Of the pair of first trials on the head movements, one yields a slight difference in one direction, the other a difference of about equal amount in the opposite direction. Neither difference has a significant statistical reliability, the first yielding a critical ratio of .33 ($r=.93$), and the second, one of .43 ($r=.89$).

The radical failure of the experiment to support the hypersuggestibility hypothesis is further emphasized by the added circumstance that it yields a marked heightening of mean suggestibility on the second of all four pairs of conditions shown in Table 2. If any generalized facilitation of the sort demanded by the hypothesis had existed, it is accordingly doubly certain that the methods of measurement in the present experiment are amply sensitive to have shown it.

Practice effects in the field of suggestion have been reportd so often that such results no longer require comment. The fact that the present data are unique in involving for the first time protracted repetition of head and arm suggestion, together with the fact that the suggestions were administered exclusively by means of a phonograph, a method never before employed on a large scale, seemed to warrant an examination of the present results from this point of view. Composite curves for first and second trials were plotted for both the head and the arm. In general, save for slight irregularities undoubtedly due to sampling errors, the arm and head showed exactly the same picture. The results of both were therefore pooled. The final composite is shown in Figure 1. This graph displays the usual reduction in suggestion time with practice, though it is somewhat atypical in not showing the tendency to become horizontal in the later days. The tendency to convergence of the lines representing the two successive repetitions confirms the somewhat similar observations made on successive repetitions of the waking postural suggestion (9). It may be noted, further, that, while the method of combining days by pairs somewhat masks the effect, a very considerable amount of the increased facility shown on the second reaction over the first is lost before the first of the period following.

Conclusions

The present investigation yields no support whatever for the hypothesis that positive response to direct suggestion as such evokes

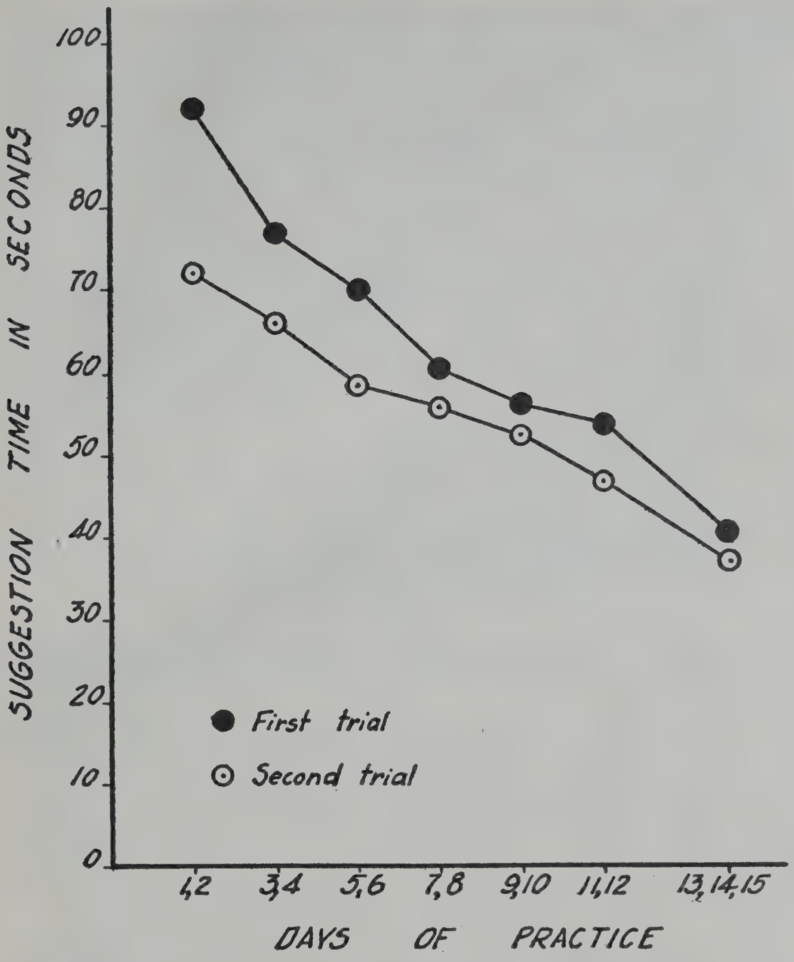

FIGURE 1

a generalized hypersuggestibility. Indeed, these results, considered in conjunction with those of Patten, Switzer, and Hull (9), indicate that there probably is no such thing as hypersuggestibility in waking suggestion. The experimental evidence so far available accordingly seems to indicate that the suggestions which bring about the hypnotic trance somehow differ in this respect from those used in the present experiment. Various possible explanations of this paradox offer themselves as starting points for future experimentation.

From the point of view of scientific method the present investigation exemplifies the principle that the function of hypotheses is to lead to decisive experiment as promptly as may be, to the end that those hypotheses which cannot stand this test may be discarded from active consideration.

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LA RÉPONSE POSITIVE À LA SUGGESTION DIRECTE COMME TELLE ÉVOQUE-T-ELLE UNE HYPERSUGGESTIBILITÉ GÉNERALISÉE?

(Résumé)

Une sensibilité accrue à la suggestion peut être un résultat particulier aux suggestions spéciales données pour faire venir l'hypnose, ou elle peut être une caractéristique généralisée de toute suggestion directe réussie au mouvement.

Si la dernière hypothèse est vraie, une sensibilité accrue doit opérer réciproquement entre deux types distincts de suggestion en état de réveil,

c'est-à-dire, une réponse à la suggestion que la tête s'incline sur la poitrine suivant une réponse à la suggestion que le bras étendu horizontalement se balance en avant, et vice versa.

On a employé treize sujets mâles que l'on avait prouvés sujets à la suggestion. On a donné à chacun deux suggestions au mouvement de la tête enregistrées sur disques phonographiques et deux au mouvement du bras chaque jour pendant quinze jours de suite selon la méthode d'alternation ABBA. On a enregistré l'étendue des mouvements au kymographe.

Cette expérience rend nulle l'hypothèse que la réponse positive à la suggestion directe comme telle évoque une hypersuggestibilité. Le temps moyen des suggestions pour le premier mouvement du bras suivant deux mouvements de la tête n'est pas plus petit que quand il précède les réponses des mouvements de la tête. La même relation se trouve pour la paire de moyennes des deuxièmes épreuves du bras.

Puisque les courbes composées de la première épreuve et de la deuxième de la tête et du bras ont montré exactement le même résultat, on a groupé les résultats ensemble. Le graphique composé final montre la diminution usuelle du temps de suggestion par exercice, mais ne montre aucune évidence d'une suggestibilité généralisée.

HULL, PATTEN, ET SWITZER

RUFT DIE POSITIVE REAKTION AUF DIREKTE SUGGERIERUNG ALS SOLCHE EINE VERALLGEMEINERTE ÜBER-SUGGERIERBARKEIT HERVOR?

(Referat)

Eine Zunahme an Empfindlichkeit der Suggerierung gegenüber ist vielleicht eine Wirkung die den besonderen Suggerierungen eigen ist, die gegeben worden sind um die Hypnose zu erzeugen; diese Zunahme kann aber auch eine verallgemeinerte Eigenschaft aller erfolgreicher direkter Suggerierung von Bewegung sein.

Ist letztere Hypothese wahr, so sollte erhöhte Empfindlichkeit reziprok wirksam sein zwischen zwei separaten Arten von Suggierung im rein wachenden Zustand, namentlich, Reaktion auf die Suggestion dass der Kopf nach vorwärts auf die Brust fallen wird, nach einer Reaktion auf die Suggestion, dass der horizontal ausgestreckte Arm nach vorwärts schwingen wird, und umgekehrt.

Dreizehn erweislich suggerierbare männliche Versuchspersonen wurden verwendet. Jedem wurden zwei phonographisch registrierte Suggestionen der Kopfbewegung und zwei der Armbewegung täglich jeden Tag an fünfzehn aufeinanderfolgenden Tagen in der wechselweisen Folge ABBA gegeben. Die Weite der Bewegung wurde kymographisch registriert.

Die Hypothese, dass die positive Reaktion auf direkte Suggestion als solche eine verallgemeinerte Über-Suggerierbarkeit (hypersuggestibility) hervorruft, wird durch diesen Versuch definitiv verneint. Die mittlere Suggestionszeit (mean suggestion time) bei der ersten Armbewegung wenn sie zwei Kopfbewegungen folgt ist nicht kleiner, als wenn die Armbewegung den Kopfbewegungsreaktionen vorausgeht. Das selbe Verhältnis besteht unter den Durchschnittspaaren bei den zweiten Armbewegungsversuchen.

Da die zusammengesetzten Kurven zur Registrierung der ersten und zwei-

ten Versuche mit Kopf und Arm genau das selbe Bild darstellen, wurden die Befunde zusammengesetzt (pooled). Die endgültige zusammengesetzte Registrierungskurve stellt die gewöhnliche Verminderung der Suggerierungszeit im Lauf der Einübung dar, liefert aber keinen Beweis für eine verallgemeinerte Suggerierbarkeit.

HULL, PATTEN UND SWITZER

EYE-MOVEMENTS DURING READING AND RECALL*

From the Psychological Laboratories of the University of Vermont

Harry Ewert

Introduction

This paper has a threefold purpose. The first is to show that the frequency of eye-movements in reading a given selection of material (poetry) decreases during a small amount of practice. The second is to show that during the recall of a given selection of material, previously read (poetry), the frequency of eye-movements is approximately equal to that observed in the original reading of such material. This would indicate that eye-movements typical of reading tend to carry over to recall. The third is to show that during recall of a given selection of material (poetry), not previously read but memorized by dictation, the frequency of eye-movements closely approximates that obtained in reading the same material. That is, under favorable conditions there is a tendency for verbal recall to evoke eye-movements which in frequency closely resemble those present while reading the same material. This seems to be true whether the material is visual or auditory.

These theses, if established experimentally, will furnish a striking example of the presence of motor components in recall. They would also indicate that eye-movements become attached to various types of symbolic processes, such as language symbols, gestures, and incipient motor reactions. Such a view agrees with Bain's theory that thought is incipient speech response and action, and with Ribot's statement that "A thought is a word or an act in a nascent state . . . a commencement of muscular activity."

I wish to go a step further and suggest that the various integral factors embodying a symbolic process are in themselves necessary for evoking such a process. That is, recall is a reaction (in our case visual-auditory-vocal or -subvocal) following integration of the various activity involved in perception. Applying this theory to our own problem, we derive the following proposition: Eye-movements accompanying verbal recall are in themselves a part of the recall.

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DERIVATION OF THE PROBLEM

Interest in this problem was aroused when the author observed eye-movements typical of reading in a student who was writing an examination paper in general psychology. The eye-movements indicated that the student was apparently revisualizing parts of her reading assignments. She would stop writing, and, while she sat quietly, her eyes were seen to move from left to right, interrupted by the typical fixations—then (apparently at the end of a line) there was large sweep back to the left (presumably to a new line) whereupon the typical saccadic movements were again resumed. Starting turned slightly upward (at the top of the page), the eyes were observed to undergo movement after movement turning slightly downward for each large sweep, until presumably a given page had been visualized, after which they again turned upward (to visualize a new page).

I was unable to find reference to the type of eye-movements here described. Consequently, I forthwith planned an investigation to determine their source, universality, frequency, and permanency under laboratory conditions.

HISTORICAL BACKGROUND

This study has two historical aspects. The one deals with the effect of practice upon the frequency of eye-movements in reading, and the other deals with the nature of eye-movements during recall. We shall consider the latter aspect first.

Two studies were found which bear more or less directly upon our investigation.[1] One is the study of Perky (30), who sought to determine the rôle of eye-movements in distinguishing between images of imagination and images of memory. She found that "in the great majority of cases, memory images of sight, sound and smell involve gross movements of eyes, larynx and nostrils, while the corresponding imaginations involve no such movements" (p. 452). Perky, however, did not attempt to analyze or trace the origin of these movements.

Another study, relating more directly to ours, is that of Stoy (35) who made "a preliminary study of ocular attitudes in thinking of spatial relations." Fifty subjects (tested individually) were asked to solve 11 spatial problems, such as: "Imagine that you are jumping up a hill. Every time you jump up 3 feet you slide back 2 feet. The hill is 30 feet long. In how many jumps can you reach the top?" and 10 non-spatial problems, such as: "All Kodaks are cameras, but all cameras are not Kodaks. Is this true or false? Why?" Eye-movements were observed with the aid of a Bausch and Lomb keratometer. The results indicate that: (1) The average of horizontal eye-movements was greater in the extent and frequency when spatial problems were solved than when non-spatial problems were solved. (2) There were individual differences in the extent and frequency of eye-movements under the two conditions of observation. But the correlations between spatial and non-spatial observations were .654 and .718 for average measure of largest movements, and frequency of movements, respectively. (3) There seems to be "evidence that eye-movements are indicative of ability to deal with spatial concepts when the latter is measured by the instructor's judgments" (p. 385). This study differs from ours in that it deals with eye-movements in a more general way. We are dealing with the perseveration of eye-movements involved in reading. Moreover, Stoy's results are made spurious by the fact that no records were obtained while the subjects were in a state of non-activity as determined by the instruction 'set.' This decidedly interrogates the significance of the obtained correlations and differences. Finally, Stoy's subjects were motivated by instructions which were supposed to initiate images of memory and imagination of a spatial and non-spatial nature. It has long been known that this type of behavior cannot be entirely controlled by the subject.

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[1] After this paper was presented for publication I discovered an investigation by Clark (3) in which she reports that "Visual images in the thinking may be closely related to ocular movements (observed by the experimenter in 32 of 74 instances) and in many instances are inseparably connected with them or with a condition of visual fixation" (p. 85).

Other studies bearing less directly upon this study are those of Erdmann and Dodge (14), Miles (24, 26, 27), Gray (18), O'Brien (29), Fardyce (17), Schmidt (34), Buswell (2, 3), Tinker (37, 38), and others, dealing with such problems as the interfixation span, frequency and duration of fixations and perception time during reading; and the studies of Curtis (5), Reed (32), Wyczoikowski (41), Clark (4), Thorson (36), Pintner (31), Rounds and Poffenberger (33), and others on the various measurements of implicit speech reactions.

Another aspect of our problem concerns the effect of practice in reading upon the frequency of eye-movements. McGinnis (23) has shown that optical nystagmus and coordinate compensatory eye-movements are present at birth. Though these movements are inherited, there is fairly good evidence that all of the five types of eye-movements described by Dodge (10) are educable.

Ocular pursuit movements, of course, might be considered modifiable in virtue of the fact that they are partially under voluntary control. However, Travis and Dodge (39) find a slight decrease in adequacy of the eye to pursue intermittently exposed moving objects in a seven-day practice period, thus showing a slight negative adaptation. These authors, however, refer to Bott, Brown, and Cohen (1) as having demonstrated the educability of convergent and divergent eye-movements. They (Travis and Dodge) say, "Bott, Brown and Cohen reported a very regular and decided improvement in a binocular motor pattern involving the depression or elevation of one eye to maintain binocular fixation; but this pattern probably involves neural relationship entirely different from those of ocular pursuit (p. 109) . . . Eye-movements play an important rôle in many of our complex mental activities and in adaptive behavior . . . The only type of eye-movement which is ordinarily initiated voluntarily is the saccadic, or the refixating movement in optic nystagmus, and once begun even this seems to pass from voluntary control (p. 101) . . . In consideration of the probable neural origins of the pursuit phase, improvement through practice could hardly be expected; but too little is known of the neural relationship involved in the whole process of ocular movement to predict the effect practice might have on particular ocular patterns, especially those including short-lived motor habits (p. 109). These quotations do not exclude the possibility of a modification of ocular patterns as a result of practice. Instead, they tend to show that improvement or change in ocular pattern is highly complex and consists of short-lived habits which arise to suit the occasion. These studies also indicate that pauses and fixations in pursuit may decrease in number without essentially changing their form. Dodge (11) has further shown that eye-movements exemplify learning of a higher order. The eye seeks out points of interest, clarity, and maximum detail in a trial-and-error fashion. Such trial-and-error reactions are, however, not entirely unguided but involve anticipatory adaptive responses which are conditioned by a given 'set.' This suggests multiformity of movement. Moreover, there are no eye-body coordinations which do not involve various types of eye-movements. Hence it is only natural to suppose that accessory eye-movements drop out during the acquisition of motor skill, in the same manner as do the unnecessary manipulatory hand, feet, and body movements.

Ewert (15) found indications that coordinate compensatory and nystagmic eye-movements become adapted to inverted vision. The swaying of the visual field and dizziness conditioned by head and body movements during inversion, which were very pronounced at the beginning of a 14-day continuous practice period, tended to disappear with practice. The author believes that such swaying and dizziness in the initial period of inversion are at least partially due to the disruption of movements which were optically induced and those of vestibular and tonic origin. It has been shown by Dodge (9) that there is no essential difference (except in latency and manner of stimulation) between nystagmus of ocular origin and that of vestibular origin. Magnus (21), Maxwell (22), DeKleyn and Magnus (7), and others find that eye-movements may also be initiated through reciprocal and tonic reflexes coming from the neck and body muscles. Ewert (in an unpublished study) observed saccadic movements following immediately and apparently aroused by a sound stimulus. During inverted vision optically induced eye-movements are thrown out of harmony with those of vestibular and tonic origin. Practice in inverted vision, however, tends to obviate dizziness and swaying of the visual field during head and body movements. Is it not possible that new reaction patterns are set up as a result of practice, tending to coordinate anew movements of optical origin with those of vestibular and tonic origin? This interpretation is in keeping with Dodge's finding that dizziness can be modified through practice, and Travis' (40) finding that eye-movements increased 50% in ocular pursuit latency when vestibular stimulation tended to turn the eye in a direction opposite that of pursuit. A 50% decrease in ocular pursuit latency was obtained when the two were in harmony.

The foregoing discussion tends to establish two important facts. The first is that eye-movements are educable. The second is that eye-movements are of diverse origin. In this study we wish to show that eye-movements may also become attached to language behavior so as to be evoked by either silent or oral verbal recall, and that such movements are apparently modifiable through practice.

METHOD[2]

The method of direct observation [a modification of Newhall's (28) method] was used in this study. A number of investigators who are thoroughly acquainted with the various measuring techniques recommend the direct method for certain types of problems. Miles and Segel (26) say, "As is well known, Erdmann and Dodge made their original investigations without recording eye-movements. They observe by the use of mirrors and also by the use of a small telescope. Freeman has devised a mirror arrangement which has apparently given good results and recently Newhall has described a simple equipment. The latter includes light, head rest, copy holder and lens for magnification of the movements to aid in directly observing them" (p. 522). Tinker (38) finds that "the results obtained by the direct method . . . show exactly the same trend as those found by the photographic method . . ." p. 196.

APPARATUS

A lens, 10 cm. in diameter, with a magnification of 10 was mounted to a stand and placed at the edge of a table. As a point of anchorage a fine hair was pasted to the lens so as to bisect it vertically. The room was illuminated by the semi-direct method with two 100-watt frosted bulbs on the ceiling. A Stoelting hand counter was used for recording the eye-movements. The reading and recall material consisted of three four-line stanzas of poetry (*John Gilpin's Ride* by Cowper) which were read in time to a metronome set at 184. There were 28 accents to each stanza. The subject's head was supported by a head-rest.

PROCEDURE

Horizontal eye-movements were recorded under the following conditions:

1. While memorizing poetry by reading it orally.
2. While memorizing poetry by reading it silently.
3. While recalling poetry by reciting it orally.
4. While recalling poetry by reciting it silently.
5. While memorizing poetry from dictation.

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[2] There is at present an investigation under way in which we (R. Wendt and Ewert) are using the photographic method of recording, namely, the mirror-reflection method. Results so obtained agree in all essentials with those of this study.

6. While recalling poetry which had been learned from dictation by reciting it orally.
7. While recalling poetry which had been learned from dictation by reciting it silently.
8. While unmotivated. The subject was facing a gray wall. No instructions were given.

For conditions 1, 2, 3, and 4 the following material was used:

Now Mistress Gilpin's, careful soul,
Had two stone bottles found,
To hold the liquor that she loved,
And keep it safe and sound.

Each bottle had a curling ear,
Through which the belt he drew,
And hung a bottle on each side
To make his balance true.

For conditions 5, 6, and 7 the following stanza was used.

The dogs did bark, the children scream'd,
Up flew the windows all,
And every soul cried out, well done!
As loud as he could bawl.

Since a metronome was used for reading as well as for recall the time was kept constant throughout the experiment. There are 28 accents to a stanza. However, we permitted a two-beat rest after every two lines. This gives us 30 beats per stanza. In reading two stanzas there are three rest periods or a total of 6 beats, bringing the number of beats for the two stanzas to 62 and the time to 21.7 secs. It is thus possible to figure roughly the number of eye-movements per unit of time.

The subject sat alongside and near the end of a table facing the adjustable lens. E sat at the end of the table facing the subject. The subject received the following instructions before each experiment:

> "You will be given some poetry to read. Read it in time to the metronome. You may stop for a breath during two beats every time you finish two lines. [E gave a demonstration at this point.] You will be asked to read the material ten times. Please start when you get the 'now' signal."

E recorded the eye-movements after each trial. After the tenth trial the subject was given 10 trials during which he read silently.

He was then asked to recite the material orally. If he could not recall the material at this stage he was asked to learn it. The instructions for recall were like those for reading except for the necessary changes of terms. Oral recall was followed by silent recall. There were 10 trials for each test condition.

A similar procedure was followed while the subject learned from dictation. E recited a stanza of poetry 10 times recording the subject's eye-movements after each trial. The subject was then asked to give 10 oral and 10 silent recitations, eye-movements being recorded after each trial. A total of 10 observations were made during which time the subject was unmotivated. The time for each observation equalled that of a reading and recall trial.

RESULTS

The total time of eye-movements during reading is largely dependent upon the frequency of such movements, since their speed is relatively constant. Furthermore, since every fixation pause is preceded and followed by a movement, it follows that the total time of fixation in reading will also be influenced by the frequency of movements, i.e., increased frequency may mean a decrease in total fixation time and vice versa. Since there is a central anaesthesia during eye-movement (19, 13, 6), perception occurs only during fixation pauses. Hence, decrease in frequency of movements means increased reading rate. It also means better comprehension, since comprehension has been found to improve with increase in time of fixation pause (26, 34, 3, 24, etc.).

Improvement in reading has been found to accompany noticeable reduction in number of fixation pauses and regressive movements. O'Brien (29) obtained records of eye-movements before and after his subjects were given drill in rapid reading, and special training to decrease vocalization. He says: "The eye-movement records show that improvement is effected physiologically chiefly by the lessening of number of fixations rather than by a shortening of the average duration of the fixation. The improvement is also accompanied by a decrease in the number of regressive movements" (p. 272). It remains a question whether O'Brien's reduction in frequency of fixation pauses during practice is actually due to practice in reading, whether it is due to the instruction to inhibit vocalization, or whether it results from a combination of both factors. I shall present data in this paper which tend to indicate that vocalization in itself may influence eye-movement rate. Furthermore, in O'Brien's study practice lasted for 39 days. I now wish to present evidence that eye-movements may decrease in the short period of 10 consecutive trials in reading two stanzas of poetry.

The data are presented in Table 1. Eye-movements were recorded while reading two stanzas of poetry. The table presents individual records for 10 subjects, each having 10 trials in oral and 10 trials in silent reading. Computed averages appear in the last vertical column of the table.

It will be observed that the average frequency of movements is

TABLE 1

THE EFFECT OF PRACTICE UPON THE FREQUENCY OF EYE-MOVEMENTS WHILE READING TWO STANZAS OF POETRY

(Time 21.7 secs.)

ORAL READING

	Subjects										
Trial	Bd	Mo	C	M	Bl	Wd	W	Mar	P	B	Av.
1	24	33	32	33	26	32	30	35	30	32	30.7
2	26	31	27	32	25	32	30	32	30	32	29.7
3	29	32	23	30	26	30	31	32	29	28	29.
4	27	31	23	28	24	30	29	31	27	30	28.
5	21	29	23	29	25	29	29	32	28	28	27.3
6	25	29	26	28	27	26	28	31	27	27	27.4
7	22	27	26	27	26	23	27	31	29	30	26.8
8	21	28	24	27	22	28	27	27	30	28	26.2
9	19	29	25	28	21	24	29	28	30	28	26.4
10	18	27	25	28	23	24	28	25	29	27	25.4
								Total Av.			27.69

SILENT READING

	Subjects										
Trial	Bd	Mo	Wd	C	Bl	M	W	Mar	P	B	Av.
1	28	38	33	24	26	24	40	40	32	41	31.9
2	26	33	30	26	25	24	38	39	32	38	31.1
3	28	34	30	26	24	22	37	31	33	38	30.3
4	27	34	30	19	26	26	32	31	28	39	29.2
5	25	34	31	22	24	24	35	29	30	35	28.9
6	23	34	30	21	25	22	30	31	32	35	28.3
7	21	33	31	25	22	23	30	30	30	37	28.2
8	23	31	31	25	21	22	30	27	32	36	27.8
9	24	32	33	24	23	21	32	26	29	33	27.7
10	21	32	32	25	22	23	31	26	28	29	26.9
								Total Av.			29.03

greater for silent than for oral reading. This is contrary to the findings of other investigators. Schmidt (34), for example, found an average of 1.7 more pauses and 74% more refixations per line in oral than in silent reading. My dissenting results are no doubt attributable to the technique employed. Reading rate was kept constant by the use of a metronome. Since comprehension rate is greater for silent than for oral reading, the allotted time (21.7 secs.) for reading two stanzas of poetry was undoubtedly more than the necessary comprehension time and hence made possible additional exploratory or regressive movements in silent reading.

The records show individual variability of movements for both oral and silent reading, thus substantiating previous findings. Such variability, however, does not appear to influence the practice effect or the reliability of our other deductions.

Records in Table 1 indicate individual as well as average reduction of eye-movements for both oral and silent reading. In examining the averages, we see that there is a constant decrease in each consecutive trial for silent reading, and that with the exception of the 5th, 6th, 8th, and 9th trials, this also holds true for oral reading. A correlation of average frequency of movements for oral and silent reading for consecutive trials, computed according to the Spearman formula, is .98±.007. This indicates a reliably constant reduction of eye-movements during reading practice.

It is thus seen that practice in reading has the effect of reducing the frequency of eye-movements while reading a given selection of material. Stated in other terms, the same set of stimuli evoke eye-movements which lessen in frequency as a result of practice. How can we account for this reduction? O'Brien states that improvement in reading is effected physiologically chiefly by the lessening of fixations. Such a statement, however, does not get at the root of the problem since it merely correlates decrease of movement with reading improvement. The vicious circle is completed as soon as we ask: What causes improvement in reading? There is evidence that some types of eye-movements are highly stereotyped. Still, repeated stimulation by constant stimuli results in reduced eye-movement frequency. Does this indicate a change in ocular pattern or does it mean that the decrease in movements has a central origin and takes place without essentially changing the overt form of the ocular pattern? The full answer to this question awaits further research. However, Buswell (3) has suggested that eye-movements adapt themselves to mental processes. Tinker (37) verifies Buswell, but adds that a "marked constancy of pause-duration in the different kinds of material as well as for different observers indicates the presence of a 'habit pause-duration tendency' . . ." (p. 113). But "this tendency to a 'pause-duration habit,' which is evident in smooth, effiicient reading, is quickly disturbed when a difficult word or phrase or a difficult kind of material (i.e., a formula concept) is encountered in reading" (p. 111). These observations indicate that practice in reading tends to mechanize ocular patterns, but that these patterns may be broken up when environmental complications arise resulting in at least partial central control. What Buswell (3) regards as mental processes would undoubtedly include the symbolic processes, consisting in the case of reading and recall mainly of language habits. It is highly probable then that the following factors play a dominant rôle in the elimination of eye-movements during practice:

1. Eye-movements constitute short-lived motor habits whose pattern results from a given environmental situation and behavior 'set.' When the situation and 'set' are altered the component ocular pattern, which is a part of the 'set,' is automatically altered as an integral part of the total behavior. In practicing reading the 'set' (vocal or sub-vocal-ocular-etc.) becomes altered by an acquaintance (language reduction) with the material. This results in reduced cues and thus in the abbreviation of ocular behavior (reduced frequency of eye-movements).
2. There is the possibility of at least a partial (central) transference from an ocular pattern to one including auditory, kinaesthetic, verbal, or other connections. Such a transference might occur with the shifting of 'set.' This seems to be a common occurrence during learning activity (20, 16).

Granted the above assumptions, we may conclude that the lessening of eye-movements during practice constitutes a physiological factor in reading improvement.

We now wish to present data tending to show that eye-movements may become conditioned to language behavior so as to be evoked through either oral or silent recall. Table 2 gives the individual and average records for eye-movements during recall of two stanzas of poetry. Each of the 10 subjects received 10 trials. Averages are presented in the last vertical column.

Considerable individual differences are evident. Subjects B and

TABLE 2
FREQUENCY OF EYE-MOVEMENTS DURING ORAL AND SILENT RECALL OF TWO STANZAS OF POETRY
(Time 21.7 secs.)
ORAL RECALL

	Subjects										
Trial	Bd	Mo	C	M	Bl	Wd	W	Mar	P	B	Av.
1	26	33	36	25	36	15	37	39	28	21	29.6
2	25	35	25	22	31	8	31	24	31	17	24.9
3	21	33	31	22	30	8	32	28	32	19	25.6
4	20	28	30	20	25	8	31	32	31	17	24.2
5	24	35	24	20	17	7	28	32	32	18	23.7
6	25	36	27	30	21	11	30	25	27	15	24.7
7	23	30	28	24	17	7	31	27	29	10	22.6
8	20	27	27	24	13	9	27	28	31	11	21.7
9	18	28	30	25	15	8	33	30	29	13	22.9
10	18	22	25	24	15	11	33	30	28	12	21.8
										Total Av.	24.17

SILENT RECALL

	Subjects										
Trial	Bd	Mo	C	M	Bl	Wd	W	Mar	P	B	Av.
1	24	30	27	25	27	10	25	28	28	11	23.5
2	20	35	21	22	24	16	23	32	31	10	23.4
3	24	34	19	22	23	10	37	35	28	12	24.4
4	25	33	21	25	18	14	33	28	26	7	23.0
5	25	30	15	27	16	19	31	33	28	7	23.1
6	22	30	13	23	13	15	30	31	27	8	21.2
7	24	32	16	24	17	14	29	30	25	6	21.7
8	22	31	17	24	13	20	30	30	26	8	22.1
9	24	30	16	25	14	20	26	27	28	9	21.9
10	24	30	15	24	14	19	30	31	29	6	22.2
										Total Av.	22.65

Wd deviate most from the averages. Their oral and silent reading records are higher than the average for the group. The averages for ten trials are 29 and 36.1 for subject B and 27.1 and 31.1 for subject Wd, in oral and silent reading, respectively. Their oral and silent recall records, however, are materially below the average for the group, namely, 9.2 and 15.7 for subject Wd and 15.3 and 8.4 for subject B in oral and silent recall, respectively. The total averages for the group are 24.17 and 22.65 for oral and silent recall, respectively. Although the physiological factors governing the frequency of eye-movements during recall are not very well known, introspective reports of subjects B and Wd indicate that dominance in sensory habits may play a rôle. Both of these subjects report that during recall their behavior seemed to be more of the kinaesthetic and auditory origin than of visual origin. It is thus possible that individual differences in ocular habits are partially due to the variation in dominance of sensory habits during learning and recall. This is in keeping with previous findings of Lambert and Ewert (20) and Ewert and Lambert (16), and others.

When the records of the two most variable subjects are omitted, the total average frequency of movements is increased from 24.17 to 27.28 for oral recall, bringing it practically up to the total average frequency of 27.69 for oral reading of the same material. That for silent recall is increased from 22.65 to 25.30. This latter figure also compares favorably with that obtained for silent reading of the same material (29.03). We thus see that the frequency of eye-movements during oral and silent recall practically matches that for oral and silent reading. This suggests that the typical eye-movements which reading necessitates tend to carry over to recall.

Such a tendency is indicated more clearly in Table 3. This table gives the average frequency of eye-movements during oral and silent reading, oral and silent recall, and during non-activity, which is given in the table under the heading *Without motivation.* The records of Tables 2 and 3 are here in part summarized for comparative purposes. The averages are obtained from 10 subjects and are

TABLE 3

Average Frequency of Eye-Movements During Oral and Silent Reading, Oral and Silent Recall, and Without Motivation

(Time 21.7 secs.)

Trial	Without motivation	Oral reading	Oral recall	Silent reading	Silent recall
1	5.5	30.7	29.6	31.9	23.5
2	6.1	29.7	24.9	31.1	23.4
3	4.8	29.	25.6	30.3	24.4
4	5.6	28.	24.2	29.2	23.
5	4.9	27.3	23.7	28.9	23.1
6	5.8	27.4	24.7	28.3	21.2
7	6.3	26.8	22.6	28.2	21.7
8	6.4	26.2	21.7	27.8	22.1
9	5.8	26.4	22.9	27.7	21.9
10	5.8	25.4	21.8	26.9	22.2
Total Av.	5.7	27.69	24.17	29.03	22.65

given for each of 10 trials. Total averages are given in the bottom horizontal column. The conditions for the various types of activity differ only in the type of motivation. The time for each trial is 21.7 secs. An examination of the data at once reveals the close resemblance in frequency of movements during reading and recall conditions. This resemblance is especially marked when the recall records for the two most variable subjects are omitted. Under these conditions there is an average difference of .41 movements per total average trial between oral reading and oral recall and an average difference of 3.73 movements per total average trial between silent reading and silent recall. These differences gain in importance when compared with the average movements occurring in an equal period (21.7 secs.) of non-activity. The total average frequency of movement per trial under these conditions is 5.7, as compared to 24.17 and 22.65 for oral and silent recall, respectively. This again suggests the conclusion that frequency in eye-movements is in some way related to language behavior. Evidently, ocular habits may become associated with language behavior in such a way as to enable the one (language behavior) to arouse the other (eye-movements). The above data indicate that eye-movements are in themselves a part of the recall. Though they are not directly under voluntary (language) control, they nevertheless play an important rôle in completing the total recall pattern. However, our arguments as to the relationship between language behavior and ocular patterns are thus far based upon the similarity of frequency of eye-movements during reading and recall and upon the fact that they increase under motivation of reading and recall as compared to non-activity. What evidence do we have that the increased frequency in recall is related to the particular recall material (poetry in this case) in question? There is still the possibility that the increased frequency during recall is evoked not by the specific recall material in question but by a more general factor. The best indication of a causal relationship between eye-movements and recall material would be obtainable from the configuration of photographic records of eye-movements under reading and recall conditions. The photographic ocular pattern for reading records is quite typically a picture of the reading material. Difference in length of line, for example, is clearly indicated in the records. Hence, if a photographic pattern typical for reading a given passage could be reproduced during recall of the same material, the argument for a causal relationship between eye-movements and recall would be materially strengthened. Such an investigation is under way and positive results will be reported in a later paper. However, in this paper we have to limit our argument to a similarity in frequency of movements.

We shall now supply data which suggest that a recall of language symbols which have been acquired, not by reading but through dictation, also result in an increased frequency of eye-movements. This would tend to strengthen the argument of an existing causal relationship between ocular behavior and language symbols. Table 4 gives the average frequency of eye-movements for 10 subjects while learning one stanza of poetry by dictation and then recalling it orally and silently. The time for each trial was 10 secs. It will be seen that the frequency of movement increases during acquisition. This indicates that, as language habits become established, they probably evoke responses in the visual center. Thus they serve to induce reactions which are normally associated with the total reaction pattern known as reading. In other words, eye-movements increase with the increasing ability to recall symbols. This again suggests that eye-movements are closely related to, or are a part of, the vocal recall pattern.

From the recall records we see that the average total frequency compares favorably with that for reading activity. By multiplying these records by 2.1 we derive a figure which is predictive of the frequency of movements in 21 seconds which is approximately the time

TABLE 4

Average Frequency of Eye-Movements While Learning by Dictation and Then Recalling Orally and Silently One Stanza of Poetry

(Time 10 secs.)

Trial	Learning by dictation	Oral recall	Silent recall
1	9.4	13.6	10.8
2	9.6	12.1	11.2
3	8.1	12.4	11.6
4	9.9	11.7	9.7
5	10.5	12.4	11.2
6	9.2	10.7	10.4
7	9.7	10.2	10.3
8	10.2	11.3	8.2
9	10.2	10.6	9.0
10	10.3	10.1	8.7
Total Av.	9.71	11.51	10.11

per trial of reading and for non-activity, since the figures given in Table 4 represent the movements during a 10-second period. By this procedure we derive the following comparative figures: For dictation, a total average frequency of 20.39; for oral recall, a total average frequency of 24.17; and for silent recall, a total average frequency of 21.33. It is thus seen that the frequency of eye-movements following the acquisition of poetry by dictation compares favorably with that following the acquisition by oral and silent reading. This is significant because it suggests that the increased frequency is not limited to specific activity such as oral and silent reading, but is evocable by auditory language symbols. This suggests a reciprocal relationship of the various receptor processes knit together by common mental (symbolic) processes such as comprehension during reading, oral or silent recall, auditory learning and recall, etc., all of which are largely dependent upon language behavior.

Interesting speculation naturally suggests itself from the above data, namely, that eye-movements might serve to evoke language behavior under favorable conditions. This is the inverse condition of what we actually found but is not supported by our data. Other problems, such as the following, remain for the most part unsolved: (1) Do all types of language symbols evoke eye-movements typical of the movements involved in the perception of such material as is characterized by the symbols? (2) To what extent is the recall reaction pattern a reinstatement of the perceived behavior pattern, i.e., to what extent may, for example, eye-movements which are a part of the reading behavior be dropped, added to, or inhibited in the recall behavior? (3) In what brain centers are the various ocular-verbal associations chiefly localized?

SUMMARY

The frequency of eye-movements were recorded under the following conditions: during non-activity, oral and silent reading, oral and silent recall after oral reading, oral and silent recall after silent reading, while learning by dictation, and during oral and silent recall after learning by dictation. An historical discussion tends to establish two important facts: the first is that eye-movements are educable; and the second is that eye-movements are of diverse origin.

From this investigation the following main conclusions are drawn:

1. Practice in reading tends to decrease the frequency of eye-movements.

2. The theory is offered that eye-movements decrease in frequency during reading as a result of a reduction of language symbols to which they have become attached and as a result of transference from ocular patterns to those including auditory, kinaesthetic, verbal, or other connections.
3. Eye-movements accompanying verbal recall are in themselves a part of the recall.
4. The typical eye-movements which reading necessitates perseverate in frequency during recall of the same material under the following conditions: during oral and silent recall after reading orally, and during oral and silent recall after reading silently.
5. Eye-movements increase in frequency while learning by dictation.
6. Recall of material which has been acquired by dictation or by reading it orally or silently enhances the frequency of eye-movements, as compared to the frequency of eye-movements derived when the subjects are not thus motivated.
7. Eye-movements may become attached to language symbols (visual or auditory) so as to be evoked through the recall of such symbols.

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LES MOUVEMENTS DES YEUX PENDANT LA LECTURE ET LE RAPPEL

(Résumé)

Au moyen de la méthode d'observation directe on a enregistré les mouvements des yeux dans les conditions suivantes: pendant la Non-Activité, la Lecture Orale et Silencieuse, le Rappel Oral et Silencieux après la Lecture Orale, le Rappel Oral et Silencieux après la Lecture Silencieuse, pendant l'apprentissage par dictée, et pendant le Rappel Oral et Silencieux après l'apprentissage par dictée.

On tire les principales conclusions suivantes:

1. L'exercice de lecture tend à décroître la fréquence des mouvements des yeux.

2. Les mouvements types des yeux exigés par la lecture persévèrent en fréquence pendant le rappel de la même matière dans les conditons suivantes: pendant le Rappel Oral et Silencieux après la lecture orale, pendant le Rappel Oral et Silencieux après la lecture silencieuse.

3. Les mouvements des yeux deviennent plus fréquents pendant l'acquisition par dictée.

4. Le rappel de la matière acquise par dictée ou par la lecture Orale ou Silencieuse, accroît la fréquence des mouvements des yeux, en comparaison avec la fréquence des mouvements des yeux qui ont lieu quand les sujets n'ont pas reçu cet exercice.

5. Les mouvements des yeux peuvent être attachés aux symboles linguistiques (auditifs ou visuels) de sorte qu'ils sont évoqués par le rappel de tels symboles.

6. Les mouvements des yeux semblent être un composant moteur d'une forme totale de rappel.

EWERT

AUGENBEWEGUNGEN WÄHREND DER LEKTÜRE UND WÄHREND DER ERINNERUNG

(Referat)

Unter Gebrauch der Methode der direkten Beobachtung wurden Augenbewegungen unter den folgenden Umständen registriert: während der Untätigkeit, des Lautlesens und des Für-Sichlesens, der mündlichen (oral) und schweigenden Wiederholung (silent recall) nach schweigender Lektüre, während des Lernens von dem Diktat, und während mündlicher und schweigender Wiederholung nach dem Lernen vom Diktat (learning by dictation).

Es werden folgende Hauptschlüsse gezogen:

1. Die Übung im Lesen pflegt die Häufigkeit der Augenbewegungen zu vermindern.
2. Die typischen durch das Lesen verursachten Augenbewegungen behaupten (maintain) ihre Häufigkeit (perseverate in frequency) während der Wiederholung des selben Stoffes unter folgenden Bedingungen: während der mündlichen und der schweigenden Wiederholung nach dem Lautlesen und während der mündlichen und schweigenden Wiederholung nach dem Für-Sichlesen.
3. Während des Lernens vom Diktat nehmen die Augenbewegungen an Häufigkeit zu.
4. Bei Wiederholung von Stoff der vom Diktat oder durch Lautlesen oder durch Für-Sichlesen erlernt worden ist, sind die Augenbewegungen häufiger, als wenn sie dann beobachtet werden, wenn die Versuchspersonen nicht auf diese Weise motiviert sind.
5. Augenbewegungen können sich Sprachsymbolen (auditive oder visuelle) so anhaften, dass sie durch die [reminiszente] Wiederholung solcher Symbole hervorgerufen werden können.
6. Augenbewegungen scheinen ein motorischer Bestandteil einer gesamten Wiederholungsgestalt (total recall pattern) zu sein.

EWERT

REINFORCEMENT AND INHIBITION OF EYELID REFLEXES*

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I. Introduction

Among the more important problems of psychology are those which treat of the modification of action, whether such modification involves the elimination of old ways of acting, the learning of new ways, or the weakening or strengthening of tendencies to action already established. Systematic studies of response modification consider such topics as growth and decline, learning, remembering and forgetting, work and fatigue, the development of personality. Depending on the experimental setting, we speak variously of reinforcement, facilitation, "stamping in," conditioning, memorizing, when the alteration of behavior is in the direction of increased response. When the alteration is in the direction of reduced response we speak of inhibition, the elimination of errors, experimental extinction, negative adaptation, fatigue, forgetting, repression.

It is an open question whether some few fundamental properties of neurophysiological systems may be discovered to embrace many of our more complex instances of the acquisition or loss of reactions. The hope of some such explanatory principles continues to lead psychologists to experiment on the borderlands of physiology. Dodge (10, 11, 12) has shown certain interesting relations between the refractory phase of the physiologists and similar human tendencies which militate against repetition of response to successively presented similar stimuli. His lead has been followed by Thorndike (33), Telford (32), Luh (27), and Allison (1) in experimental application of the principle to reactions at other than reflex levels.

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[1] Some of the results of this study were presented before the National Academy of Sciences, New Haven, Connecticut, November 18, 1931. Grateful acknowledgment is due Professor Raymond Dodge of the Institute of Human Relations for the use of equipment and for aid and encouragement throughout the investigation.

Inhibition as a common phenomenon in human action has been reviewed by several writers. Dodge (9, 10) has studied the possibilities for psychology of theories derived largely from physiological experimentation, supplemented by his own studies based on human subjects. Cobb (4) has brought together the treatments of reflex physiology and related them to the problems of psychiatry. Fearing (18) gives a historical chapter to inhibition, and Skaggs (31) devotes a book to the major forms of inhibition in man, with primary attention to the problems of retroaction and other inhibitions in memory. Chappell (3), sensing the wide use of the term, feels that psychologists have been somewhat hasty in their explanations of inhibitory phenomena.

The supplementary concept, that of reinforcement or facilitation, has not received similar systematic treatment, although any number of experimental facts are available from the study of reflex and voluntary action, motivation, ergography, learning. Peak (28) has given an admirable outline summary of the facts as they relate to the reinforcement and inhibition of reflex response. The reason for the overemphasis on inhibition as compared with facilitation may lie in its greater mystery. Reinforcement seems to be easily explained in terms of summation of stimuli, concurrent response tendencies, overlapping fields of excitation, and the like. The explanation of prolonged inhibitory states is more difficult.

In the experiments to be reported, there are analyzed certain interacting effects on human reflex response of relatively discrete stimuli. The experiments throw some light on the similarity of reinforcement and inhibition phenomena in the human subject to these phenomena revealed in physiological experiments on spinal animals. They suggest certain factors governing the modification of behavior through antecedent adjustments or preparatory set. A faint stimulus, itself eliciting only minimal reflex reactions, may greatly exaggerate or depress the response to a more intense stimulus which follows it. By slight changes in the interval of time between the first and second stimulus, the reinforcing effect may be converted into an inhibitory effect. The following study deals with the reinforcement and inhibition of the eyelid reflex to sound by a light stimulus presented at various intervals before a sound stimulus, and with the corresponding effects on the reflex to light when the sound was presented first.

II. Apparatus

The experimental set-up was essentially that used in previous experiments on conditioning the eyelid reaction (23). A number of improvements were made possible after moving to the new building of the Institute of Human Relations, where a vibrationless pillar was available for mounting the instrument. The secondary cues, already subliminal, were reduced by minor technical changes in the stimulating devices. Eyelid responses were photographed by the shadow method of Dodge, utilizing his pendulum-photochronograph (8).

The essential features of the apparatus follow:

1. *Pendulum-photochronograph.* A pendulum in the course of its path broke electrical contacts which presented the stimuli at predetermined intervals. The pendulum system also carried the sensitive paper on which the stimuli and responses were photographically recorded.

2. *Primary light source.* A 100-c.p. Ediswan Point-o-lite provided a source of light for producing the shadows by which photographic registration was made.

3. *Cylindrical lens and grid of threads.* The lens focussed the light to a vertical line on the photographic paper. The shadow of the grid of silk threads across the lens produced a series of lines 1 mm. apart on the record. These served as guide lines for measuring the extent of lid motion.

4. *Timing device.* A toothed wheel interrupted the light source every 5 σ, producing time lines as ordinates on the record. Every tenth tooth was emphasized, accentuating ordinates at intervals of 50 σ. The shadows of the threads and the time lines together gave a cross-sectioned appearance to the records.

5. *Artificial lash.* An artificial eyelash of paper 1.5 mm. by 20 mm. was attached just above the natural lashes. The shadow of this paper lash across the lens gave an accurate record of lid motion.

6. *Fixation object and pre-stimulation field.* The fixation object was a black cross 2 cm. by 2 cm., arms 2 mm. wide, fastened on a plate of milk glass illuminated from the rear. The illuminated surface provided a pre-stimulation field 8 cm. wide and 9 cm. high, at a distance of 70 cm. from the eyes of the subject. The apparent brightness of this field was .04 foot-candles, as measured with a Macbeth illuminometer.

7. *Head rest.* The head was maintained in position for fixating

the cross and for holding the paper lash in the beam of recording light.

8. *Sound stimulus.* A magnetically released wire snapper activated by a spring produced the sound stimulus as it struck a wooden box resonator. Recording was by means of the shadow of a direct lever offset from the resonator.

9. *Light stimulus.* A sudden illumination increase around the fixation object constituted the light stimulus. This increase in illumination was produced by the release of a pendulum shutter swinging before the filament of a 150-watt incandescent lamp enclosed in a light-proof box behind the milk-glass fixation surface. The apparent brightness of the surface when illuminated by the stimulus light was measured as 7.0 foot-candles. The moment of stimulation was directly recorded by deflecting a portion of the stimulus light to the lens and photographic paper. It shows on the record as a darkening of the upper portion of the record when the light flashes on.

The altered stimulus light was the main difference in the apparatus from that used in the conditioning study (23), in which the light was reflected to the eyes of the subject from a sanded tin surface. In the present experiments the illumination change was transmitted through a milk glass. The apparent brightness of the light was approximately doubled, and the fixation surface was brought 100 cm. closer to the eyes. The latency of the reflexes to light was reduced as a consequence. In the conditioning study the mean latency of 748 reactions was 110.7 σ (*S.D.* 13.2 σ). For the 185 normal reflexes in the present experiments the mean latency was 95.5 σ (*S.D.* 9.8 σ). The reduction of latency with increased intensity helps to account for the disparity between the latencies as obtained and the latency of 88 σ reported for the reflex to light by Zwaardemaker and Lans (39).

Specimen records are reproduced as Figure 1. They are reduced from the original size of 2.5 x 7 inches, and some of the details are lost in the reproduction. However, the incidence of the stimuli and the configuration of the responses may be noted on the reproduced records.

III. Procedure and Controls

Reaction to light and sound were recorded for each of 12 time intervals between stimuli. A single presentation consisted of one light stimulus and one sound stimulus, separated by the predetermined interval of time. At some intervals sound preceded light, at others light preceded sound, and at one light and sound occurred simultaneously. The intervals by which sound preceded light (designated S-L intervals) were 25, 250, and 300 σ. The interval of simultaneity is designated as 0 σ. The intervals by which the light preceded the sound (L-S intervals) were 25, 50, 75, 100, 125, 200, 350, 450 σ.[2]

The intervals were arranged in a controlled random order, repetitions of presentations at a given interval always being separated by presentations at one or more other intervals. The average position in the series was approximately the same for the ten presentations at each interval. The equal average order tended to counterbalance the influence of negative adaptation during the session. The random order served also to reduce the tendency for conditioned responses to appear and confuse the results.

The presentations were given in series of 10 trials, with the presentations in the 10-trial series spaced 30 seconds apart. There were 2-minute rests between the 10-trial series. By giving 6 such series on each of two days, 10 records were secured of the reactions of each subject at each of the 12 intervals between light and sound. Reactions from 10 subjects are reported. The subjects were advanced undergraduates, graduate students, and research assistants.

Breathing instructions were given to the subject and the stimuli presented while the breath was held. The method is that of Dodge, and has been defended elsewhere (23). Miss Peak (28) has shown the importance of controlled set in experiments on the lid reflex.

The recording device gives an accurate registration of stimuli and responses; the controlled procedure makes possible valid conclusions from the records obtained.

IV. Reinforced and Inhibited Reactions as Recorded from Ten Subjects

The four reproduced records of Figure 1 show characteristic responses at four of the twelve stimulus intervals, and serve to illustrate the results later to be presented in tabular form. All records

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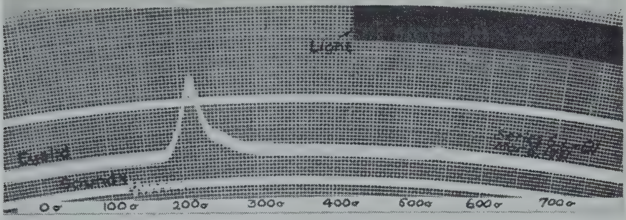
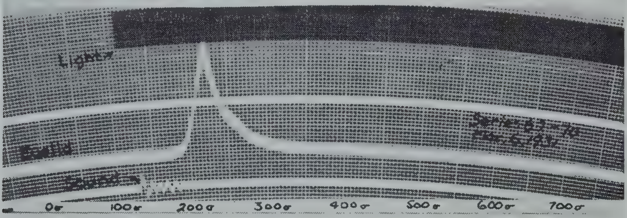
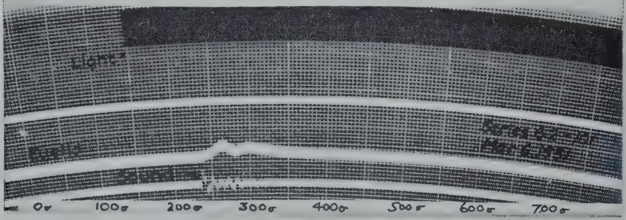
[2] The actual intervals, as read from the records, vary slightly from these round numbers. The variation is never over 5 σ. There is no overlap of intervals, and, for convenience in presentation, it is satisfactory to designate the intervals as above. Wherever the intervals enter into calculation, the actual intervals, as read from the records, have been used.



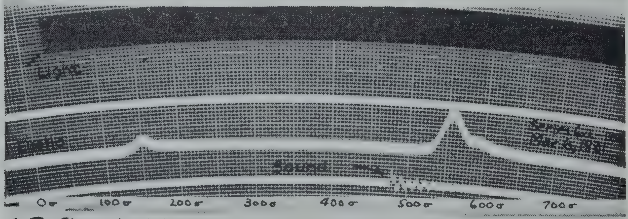
1. Reflex to Sound normal; reflex to Light inhibited



2. Augmented reflex to Light and Sound



3. Reflex to Light normal; reflex to Sound inhibited



4. Reflex to Light normal; reflex to Sound recovering

FIGURE 1

Records Showing Normal, Reinforced, and Inhibited Responses

For explanation of records, see text. In the top record, sound precedes light. In the three other records, light precedes sound, by increasing intervals.

are from the same day's session with a single subject (second day, Subject B_1). They will be considered in order.

1. *Stimulus interval S-L 300 σ. Sound precedes light. (Figure 1, Record 1.)* The break in the lower line indicates the sound stimulus, the darkening at the top of the record the flashing on of the light. A rise of the eyelid shadow indicates lid closure, a downward stroke indicates return of the lid to the normal open position. Since the sound occurred first, the first reflex is to the isolated sound, uninfluenced by the light. This is representative of what will hereafter be termed "normal" responses to sound. The reflex reaction to light is distinguishable as a slight break in the eyelid line after the light. This reaction is reduced below its normal amplitude. The inhibited reaction is to be compared with the normal reflexes to light in Records 3 and 4.

2. *Stimulus interval L-S 25 σ. Light precedes sound. (Figure 1, Record 2.)* The only reaction to be distinguished on this record occurs at a normal latent period for a reflex to sound. Its amplitude is augmented because the light preceded the sound by a sufficient margin. The amplitude of the reinforced reflex is greater than that which would be obtained by adding the amplitude of the normal response to light to that of the normal response to sound.

3. *Stimulus interval L-S 100 σ. Light precedes sound. (Figure 1, Record 3.)* Careful examination of the record reveals a dicrotic response, the first component being the reflex to light, the second the reflex to sound. The reflex to light is of normal amplitude, since it is a response to the isolated light stimulus; the reflex to sound is greatly inhibited due to the occurrence of the light.

4. *Stimulus interval L-S 450 σ. Light precedes sound. (Figure 1, Record 4.)* Here the two reactions are again separate. The normal reflex to light occurs first, the recovering reflex to sound second, towards the right on the record. Comparison with Record 1 shows that the reflex to sound has not fully recovered its normal amplitude.

With reference only to the reaction following sound, the records of Figure 1 show three stages: (1) reaction to sound normal when sound precedes light, (2) reaction to sound[3] reinforced when light precedes by short intervals, (3) reaction to sound reduced when light precedes by slightly lengthened intervals. The limits of these stages may be determined from the results for the 12 stimulus intervals.

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[3] The augmented reflex following sound in Figure 1, Record 2, is a combined response to light and sound. For convenience, the combined reactions in which the two components cannot be distinguished are called reinforced reactions to sound. This designation causes no difficulties until the latencies of combined reactions are treated, when it is important to know whether the initial rise is a reaction to sound or a reaction to light.

In Table 1 are presented the results obtained from 10 subjects. The subjects are divided into two groups of 5 each, Group A with normal reflexes to sound of much smaller amplitude than those of Group B. Presentation of results according to the two groups reduces the standard deviations which would be obtained if they were treated together. The number of records treated at each interval varies somewhat because records in which secondary

TABLE 1

Amplitude of Normal, Reinforced, and Inhibited Reflexes to Sound at Twelve Stimulus Intervals between Light and Sound

	Group A 5 subjects			Group B 5 subjects			Remarks
	n	Mean -mm.	*S.D.* -mm.	*n*	Mean -mm.	*S.D.* -mm.	
Sound preceded light by							
300 σ	50	1.8	2.3	46	26.3	10.0	Normal
250 σ	47	1.2	0.9	49	25.1	9.2	Normal
25 σ	44	1.5	1.3	47	24.3	10.0	Normal
Average for the 3 S-L intervals: normal to sound	141	1.5	1.7	142	25.1	9.7	Normal*
Light preceded sound by							
0 σ	49	2.0	1.9	45	25.1	10.1	Normal
25 σ	44	2.3	1.8	46	31.9	5.2	Reinforced
50 σ	49	5.1	5.5	44	31.9	9.5	Reinforced
75 σ	51	2.9	3.5	48	23.0	13.0	A-reinforced B-inhibited
100 σ	48	1.1	1.0	47	13.1	11.8	Inhibited
125 σ	36	1.1	0.7	45	10.4	9.3	Inhibited
200 σ	40	1.3	0.8	49	10.2	6.8	Inhibited
350 σ	51	1.3	0.7	43	14.0	9.7	Inhibited
450 σ	35	1.8	1.0	45	14.2	8.1	A-recovered B-inhibited

*These averages are used in Figure 2 as normal values of the reflex reactions to sound. All amplitude values in this and succeeding tables are in millimeters of lid motion as read from the records, at a magnification of 3 to 4 times the lid motion at the eyeball. Magnification increases slightly at the greater amplitudes.

responses occurred were discarded as ambiguous, and extra records were occasionally secured through errors of the experimenter in the order of presentation of stimuli. The general consistency of results when averaged from the records grouped by stimulus intervals, and, when averaged from the means for each subject (Table 3, later), indicates the reliability of results.

The values of Table 1 and Figure 2 show that the effect of the light on the lid reflex to sound is noticeable whenever the light precedes the sound. The reflex to sound is approximately normal when the light and sound occur together.

Reinforcement begins when the light precedes the sound by 25 σ, and reaches a maximum at either the 25-σ or the 50-σ interval. The maximum appears to be reached earlier by the subjects of Group B, with the larger mean amplitudes. How the reinforcement may be accounted for in terms of the overlapping of the tendencies to react to light and to sound will be explained later in relation to the temporal characteristics of the two responses.

Inhibition sets in when the light precedes the sound by intervals between 50 σ and 100 σ. For Group A inhibition is not found until the 100-σ interval, while for Group B it has begun at the 75-σ interval. The standard deviation of the amplitude of response for Group B is at a maximum at the 75-σ interval. Examination of the records shows this to be a critical interval: in some reactions facilitation is still marked, while in other reactions of the same subjects, inhibition is pronounced. The average value for Group B, which closely approximates normal, masks these extremes. Inhibition continues at the increasing L-S intervals. Responses at the 350-σ and 450-σ intervals show recovery, not yet to normal amplitude for Group B, though to normal (or slightly supernormal?) for Group A. The conditions favoring inhibition will be further studied later in this paper.

The amplitudes of reflexes to light are summarized in Table 2. The normal reflexes to light of the intensity used in these experiments are of much smaller amplitude than the normal reflexes to sound. The inhibited reflex to light can be studied satisfactorily only from records of the presentations made at S-L intervals sufficient for the lid to have returned to normal position following sound before the light occurs. This was true at the 250-σ and 350-σ intervals. Results are shown in the table. The reflex for Group A, already minimal, was completely inhibited, while for Group B it was greatly reduced in amplitude. The effect of sound preceding light is comparable to the effect of light preceding sound.

The summarized results of the two groups of subjects show sufficient comparability to indicate similar processes responsible for the reactions of the two groups. There are, however, differences in the reactions of individual subjects which are lost in the curves of average performance. The results by subjects are presented in Table 3 and plotted individually in Figure 3. The boldfaced figures in the table indicate the intervals of maximum and minimum response for each subject. Surprising uniformity is found in the

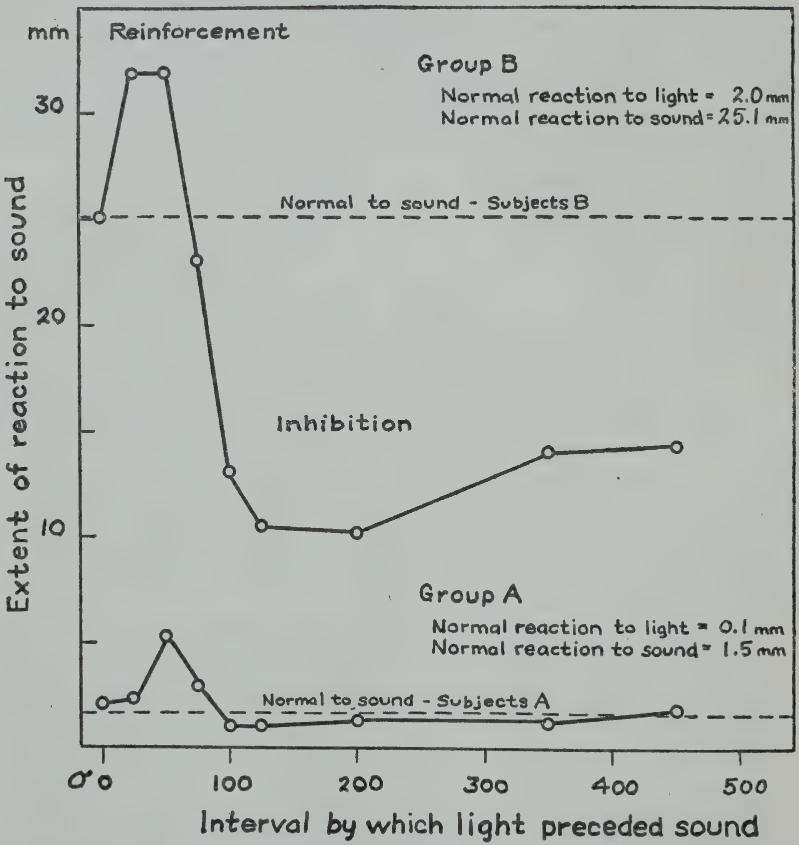

FIGURE 2

Effect of Light on Amplitude of Reflex to Sound

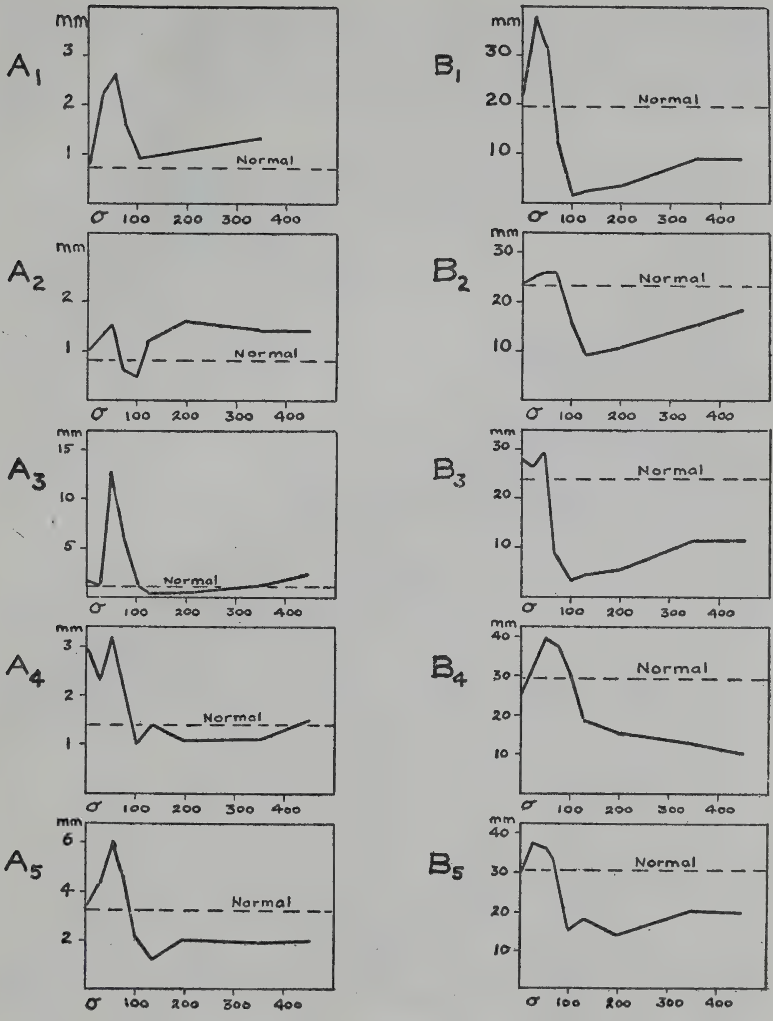

FIGURE 3

Effect of Light on Amplitude of Reflex to Sound for Each of Ten Subjects

(Note especially the differences of Subjects A_2 and B_4 from the rest.)

interval of maximum response, 8 out of 10 subjects showing the maximum at an L-S interval of 50 σ, and two at 25 σ. The interval of minimum response is 100 σ for 5 subjects, 125 σ for 3 subjects, and 200 σ and 450 σ for one subject each. These results

TABLE 2

AMPLITUDE OF NORMAL AND INHIBITED REFLEXES TO LIGHT

	Group A 5 subjects			Group B 5 subjects		
	n	Mean -mm.	*S.D.* -mm.	*n*	Mean -mm.	*S.D.* -mm.
Normal reflexes to light (light preceded sound by 125 σ, 200 σ, 350 σ, 450 σ)	162	0.1	0.4	182	2.0	3.1
Inhibited reflexes to light (sound preceded light by 300 σ, 250 σ)	97	0.0	0.0	95	0.3	0.9
Decrease in mean amplitude		0.1			1.7	
Percentage of inhibition		100%			85%	

TABLE 3

AMPLITUDES OF NORMAL, REINFORCED, AND INHIBITED REFLEXES TO SOUND FOR EACH OF TEN SUBJECTS

			Mean amplitude of response — mm. Reflexes to sound at each L-S interval								
Subject	Normal to light	Normal to sound	0 σ	25 σ	50 σ	75 σ	100 σ	125 σ	200 σ	350 σ	450 σ
A$_1$	0.0	0.7	0.8	2.2	**2.6**	1.6	**0.9**	—	—	1.3	—
A$_2$	0.1	0.8	1.0	1.3	**1.5**	0.6	**0.5**	1.2	1.6	1.4	1.4
A$_3$	0.3	1.0	1.7	1.2	**12.8**	5.4	1.0	**0.3**	0.4	1.1	2.6
A$_4$	0.0	1.4	3.0	2.3	**3.2**	2.4	**1.0**	1.4	1.1	1.1	1.5
A$_5$	0.2	3.3	3.4	4.4	**6.3**	4.8	2.2	**1.4**	2.0	1.9	2.0
Mean for Group A	0.1	1.4	2.0	2.3	**5.3**	3.0	**1.1**	**1.1**	1.3	1.4	1.9
B$_1$	3.9	19.1	21.4	**37.4**	30.8	12.5	**1.3**	2.2	3.3	9.5	8.9
B$_2$	2.0	22.9	23.3	24.4	**24.7**	**24.7**	15.9	**9.6**	10.7	15.1	18.3
B$_3$	0.4	23.5	28.0	26.5	**28.7**	8.4	**3.3**	4.1	5.8	11.8	11.8
B$_4$	3.0	28.6	25.1	32.7	**38.8**	37.2	30.4	18.0	16.4	13.0	**10.1**
B$_5$	0.3	30.9	29.3	**36.6**	35.1	33.0	15.6	18.1	**13.5**	20.7	20.4
Mean for Group B	1.9	25.0	25.4	31.5	**31.6**	23.2	13.3	10.4	**9.9**	14.0	13.9

The boldfaced figures indicate intervals of maximum and minimum response for each subject. Note maximum at L-S intervals of 25 σ and 50 σ, and minimum irregularly from 100 σ to 450 σ. The values are plotted in Figure 3. The mean values for responses of Group A and Group B differ slightly from those of Table 2, because these are averages of averages, and the same number of records was not available for each subject.

point to greater uniformity in this situation than Forbes, Querido, Whitaker, and Hurxthal (19) or Eccles and Sherrington (15) found in their two stimuli situations in experiments with spinal animals, while some of the deviations show constructive parallels to their results.

Of the anomalies revealed by the individual curves, several deserve mention. Subject A_1, with the smallest normal winks, was the only subject who failed to show inhibition, although the general configuration of the curve is similar to the others. Four out of five subjects of Group A have slightly supernormal responses at the longest interval tested. Subject A_2 showed the usual reinforcement at 50 σ , and the usual onset of inhibition at L-S intervals of 75 σ and 100 σ, but exhibited sudden recovery to a supernormal amplitude at 125-σ and longer intervals. Notwithstanding the small amplitudes involved, the results were consistent both days of experimentation, and may be regarded as reliable. These results of Subject A_2 give the only suggestion that two processes, of briefer and greater duration, may be involved in the inhibitory phase of the curves. The reinforcement-inhibition curves of Subjects B_1, B_2, B_3, B_5 are of similar pattern, while the curve of Subject B_4 reveals variations. Reinforcement endures for Subject B_4 through the 100-σ interval, longer than for any other subject, and inhibition becomes more pronounced with increase in interval, to the longest interval tested. Apparently both the reinforcement and inhibition phases are of longer duration for this subject than for any of the others tested.

V. Conditions under Which Reinforced and Inhibited Responses Occur

Reinforcement was found to occur at L-S intervals between 0 σ and 75 σ, with maximum reinforcement for the subjects of Group A at 50 σ, and for the subjects of Group B at 25 σ and 50 σ. One possible basis on which to explain these maximum points is the overlap of the reaction tendencies when the stimuli are presented at an interval equal to the difference in the latencies of their respective reactions. Since the latency of the reflex to light is greater than that of the reflex to sound, the light stimulus must precede the sound in order for the response tendencies to occur simultaneously.[4]

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[4] Various simultaneities may be distinguished: physical simultaneity, as of the stimuli, physiological simultaneity, as in concurrent response tendencies, psychological simultaneity, involving judgment about the time of

TABLE 4
Latency of Normal Reflexes to Light and Sound

	Group A			Group B		
	n	Mean	*S.D.*	*n*	Mean	*S.D.*
Latency of reflexes to light	56	96.0 σ	6.5 σ	129	95.3 σ	10.9
Latency of reflexes to sound	139	43.4	6.9	141	37.6	4.8
Difference in latency		52.6 σ			57.7 σ	

The latencies of reaction given in Table 4 show that the difference in latency between reflexes to light and sound is 52.6 σ for Group A and 57.7 σ for Group B. Stimulus intervals of these values should theoretically provide coincident response tendencies, and hence facilitation. The intervals near these values prove to be facilitory, and although no evidence is available to determine precisely on which side of 50 σ the point of maximum reinforcement lies, these values may be the intervals of maximum reinforcement.

The maximum is certainly nearer to 50 σ than to 25 σ or 75 σ, and hence it may be stated that the region of approximately simultaneous response is the favored condition for reinforcement.

Absolute simultaneity of response is not a prerequisite for augmented reaction, and the duration of the period of reinforcement is one of the interesting problems. In order to reconstruct the response characteristics at each stimulus interval, two time measures have been taken for each response. The first of these is latency, that is, the time elapsing between the stimulus and the first departure of the lid shadow from the base line. The second measure, called time to maximum, is the time elapsing between the beginning of closure and the point of greatest amplitude. The values for all stimulus intervals are presented in Table 5.

The interpretation of the latency values and times to maximum of Table 5 is aided by a study of Figure 4 in which response curves are reconstructed on the basis of the time and amplitude values of

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occurrence of stimuli. It is suggested that *physiological simultaneity* in this study is provided by a stimulus interval equal to the difference in latency between reactions to light and sound. Since reaction tendencies show temporal dispersion, physiological simultaneity may have appreciable duration.

the responses of Group B. (The values may be found in Tables 3 and 5.) At each interval, three reactions are plotted, reflexes to light and sound as they would occur to the stimuli separately presented at the given time values, and the combined reaction actually obtained when the stimuli were presented together.

At the 0-σ interval, reactions to the combined stimuli are similar to reactions to sound alone. The response to light is lost in the

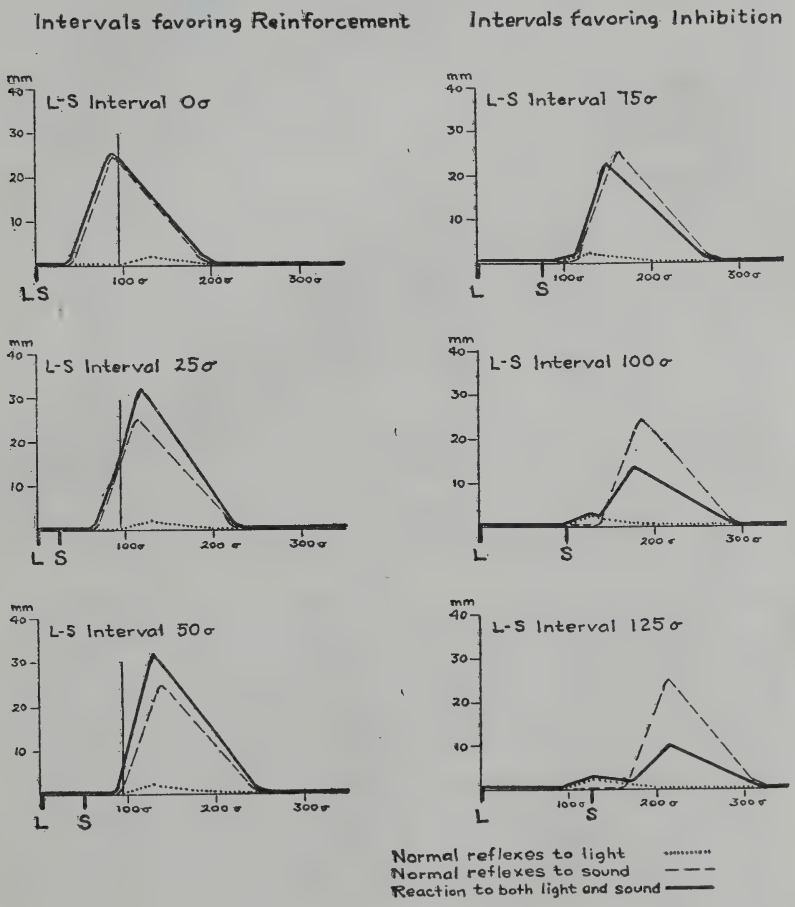

FIGURE 4

Analysis of Conditions of Reinforcement and Inhibition

TABLE 5

LATENCY AND TIME TO MAXIMUM OF REFLEXES TO SOUND AT ALL STIMULUS INTERVALS

	Group A		Group B	
	Latency	Time to maximum	Latency	Time to maximum
Sound preceded light by				
300 σ	42.4 σ	36.2 σ	37.0 σ	50.3 σ
250 σ	44.4	31.0	36.9	50.6
25 σ	41.9	35.8	37.4	50.9
Average for the 3 S-L intervals: normal to sound	42.9 σ	34.3 σ	37.1 σ	50.6 σ
Light preceded sound by				
0 σ	43.3 σ	35.7 σ	37.6 σ	50.2 σ
25 σ	42.7	42.0	37.6	52.4
50 σ	40.9	41.2	33.1	48.6
75 σ	31.2*	40.0	36.6*	37.2
100 σ	37.7*	33.9	33.7*	42.8
125 σ	41.5	34.1	39.3	43.5
200 σ	41.7	33.4	39.0	45.6
350 σ	40.9	37.7	40.5	49.1
450 σ	39.7	40.8	40.4	48.1

*These low latencies at inhibited intervals are due to the inability to distinguish between the reaction beginning to light and that beginning to sound. See Figure 4.

The latencies of normal reflexes to sound differ slightly from the values of Table 4 because these are averages of averages, whereas the values of Table 4 were determined directly from all the records available.

descending limb of the curve of the reflex to sound. The curves are constructed like the photographic records, a rising slope indicating lid closure. Reinforcement is shown to occur at L-S intervals of 25 σ and 50 σ, when the tendency to react to light occurs during the rising phase of the reflex to sound. The reaction begins as a response to sound, and is continued by the response to light. At the 50-σ interval, the overlapping tendencies result in reduced latency of reaction. The reduction in latency is not simply a function of the increase in amplitude, for the reduction does not occur at the 25-σ interval, where the amplitudes are as great as at the 50-σ interval.

Inhibition sets in at the first interval in which the reaction begins to light and is continued as a reaction to sound. Inhibition is apparent even when the reaction to sound begins in the rising phase of the reaction to light. The reduction in response increases as the reaction to sound falls later in the reaction to light, e.g., during the return of the lid to normal.

Intervals favoring summation have been found near, but not always at, the point of simultaneous response tendencies, by Eccles and Sherrington (14). Eccles and Granit (13) found the favorable position for facilitation when a second response was intercurrent with an earlier response in the rising phase (recruitment) of the crossed extensor reflex. These results agree substantially with the findings of the present experiment. A similar time relation was found by Wendt (35) to be favorable for reinforcement of the human knee-jerk by secondary impulses from the opposite leg. From these facts one would expect reinforcement whenever one reflex reaction occurred in the rising phase of the other. This principle is limited by the fact that inhibition set in for the subjects of Group B when the response to sound occurred during the rising phase of the reaction to light. This must remain for the present unaccounted for. It may be that the reflex to light is made up of less synchronous impulses than the reflex to sound, and inhibitory processes are getting under way before the maximum is reached.

The fact that the amount of reinforcement by the light is greater than the amplitude of the reflex to light remains unexplained. Both mechanical and physiological hypotheses might be suggested. There are mechanical reasons why a second response occurring during the course of one already begun might contribute more than its normal amount to the total lid motion. An isolated stimulus must overcome the inertia of the resting lid, so that the total excursion due to mild stimulation might be restricted by the impediments to starting a response. The slow initial rise just before maximum acceleration in the reflex records suggests such explanation. A second set of impulses, finding the lid in motion, might therefore contribute an amount of reinforcement greater than the reaction which the second impulses could alone provoke. These mechanical factors probably contribute less to the augmentation of the responses than certain other factors related to the physiology of reflex response.

That the concurrent effect of two different stimuli may be greater than the sum of two weak reflexes to these stimuli presented separately has been demonstrated by Forbes and his co-workers (19), Denny-Brown and Sherrington (6), Eccles and Sherrington (14).

These investigators agree that the augmentation of response due to the second stimulus results chiefly from the stimulation of units which did not respond to the first stimulus. When a muscle is stimulated by way of an afferent nerve, certain of its motor units are activated, while others are stimulated to subthreshold degrees. A second volley of impulses may stimulate certain other units, which would constitute its independent contribution to the muscular contraction. But the second volley may also stimulate to above-threshold values some of the units of the "subliminal fringe" of the first volley. This explanation of reinforced responses in terms of overlapping "subliminal fringes" is advanced by Denny-Brown and Sherrington (6, 30). By an ingenious use of the "back-firing" method of antidromic volleys, Eccles and Sherrington (15) have demonstrated that reinforcement of the flexor reflex of the spinal cat by a second centripetal volley of impulses is due primarily to the response of new units and not to the repetitive discharge of units responding to the first stimulus. While some repetitive responses occur, evidence seems to favor an explanation in the Eccles and Sherrington study largely in terms of the overlap of subliminal fringes. Such processes are no doubt operative in the reinforcement of the lid reflex to sound by the faint light stimulus.

The inhibitory period is of long duration, not being ended for the subjects of Group B at an interval of 450 σ after the light stimulus. The phase of depressed response at these long intervals between stimuli corresponds to the reflex refractory phase reported previously for the lid reflex to two lights by Zwaardemaker and Lans (39) and to two sounds by Dodge (7, 11) and Cohen (5). The refractory phase of the responding neurons is relatively short, since complete recovery is found in 10 σ to 15 σ (Eccles and Sherrington, 15). Refractoriness of the responding units might account for the onset of inhibition, but further hypotheses are needed to account for its long duration. Among factors suggested to account for the longer phase of depressed response, which is found also in the reflexes of spinal animals, are the "line-busy" effect and "equilibration" or "fatigue" of Gerard and Forbes (22), and an active inhibitory process similar to the inhibition by the contralateral afferents in cases of reciprocal innervation (16). Gerard and Forbes found the phase of decrement to last at least 800 σ, and Eccles and Sherrington have found it to last upwards of 400 σ. All agree that the long-lasting inhibition must be something other than the refractory phase of the responding elements.[5] One reason for distinguishing types of inhibition is that some preparations show only a brief period of inhibition with quick recovery (like Subject A_2, Figure 3), while others show the long-lasting inhibition. Forbes and his co-workers (19) and Eccles and Sherrington (15) have shown that the change from one form to the other may take place during the course of the experiments on one preparation.

The data of the present experiments give no direct information on the nature of the inhibitory process, except to throw some light on its amount and duration. Whatever the more intimate nature of the inhibitory processes may be, these processes with which we are dealing correspond to the enduring inhibitions described by the physiologists cited above. Eccles and Sherrington (16) are favoring a view of inhibition in terms of an enduring central inhibitory state (c.i.s.) corresponding to and differing from the central excitatory state (c.e.s.) with which it interacts. The view is an outgrowth of Sherrington's "chemical theory" reviewed by Fulton (20) in his discussion of theories of inhibition. Fulton (21) disagrees in certain details with Eccles and Sherrington, but they agree that the Wedensky effect (inhibition by overcrowding) is untenable as explanatory of the longer enduring inhibitory states. For a recent vigorous defence of the Wedensky type of inhibition, see Holt (25).

Dodge's emphasis on the importance of refractory phase in human behavior does not depend for its argument on the truth or falsity of one or the other theories of reflex inhibition. It is based on data similar to those of the present experiment, in which a faint stimulus greatly modifies the reaction to a more intense stimulus which follows it. Refractory phase as used in this sense does not refer to repetitive responses in given motor neurons, but to the general fact of decrement in reaction to a second stimulus following reaction to a first.[6] Regardless of theories of inhibition, the fact of such decrement is true.

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[5] Physiologists seem to be limiting the term refractory phase to the period of unresponsiveness in single elements following response (16, 17, 22). The literature is not clear on this point. There are many conditions under which a period of raised threshold or lessened response follows stimulation, and these periods are not uniformly called refractory. Even for nerve fiber there are after-effects of certain types of stimulation resulting in reduced response which Erlanger and Blair distinguish from a refractory period by calling a "phase of depression" (17).

[6] The reaction to the first stimulus need not be overt for this decrement to occur.

Thus far the degree of inhibition has been described primarily in terms of amplitude of response. Latency of response might also be expected to yield information. The values of Table 5 show that inhibited responses (for Group B) have longer latencies than facilitated responses. The longer latency of the normal reflexes for Group

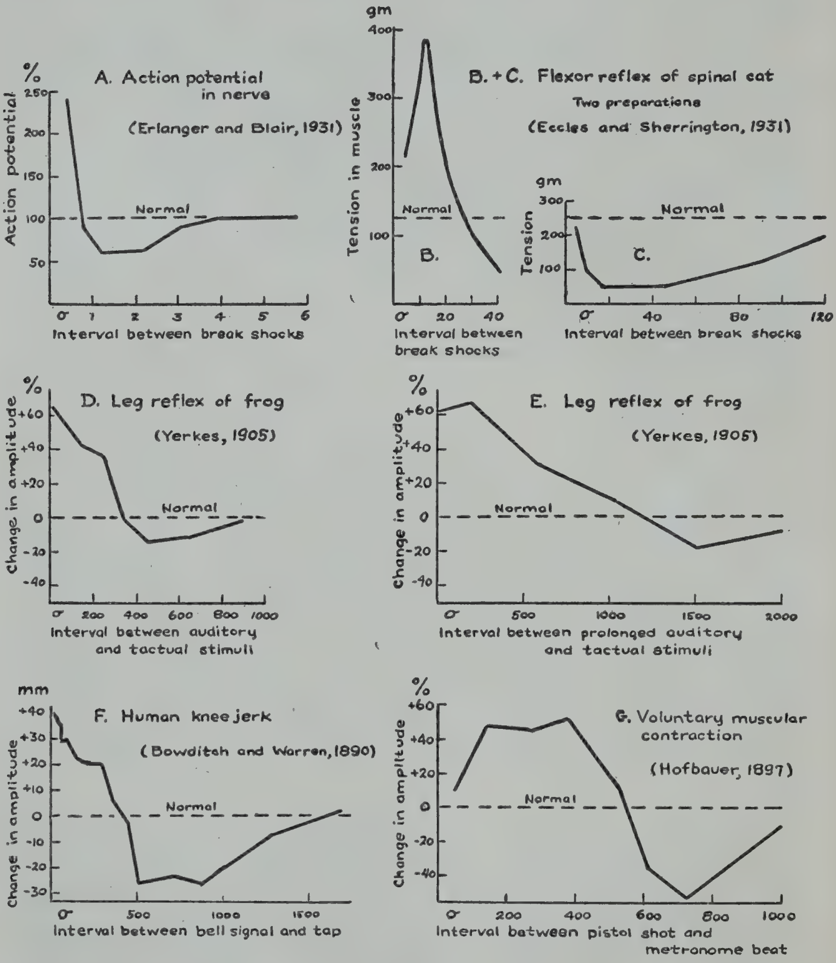

FIGURE 5

THE RESPONSE TO A SECOND STIMULUS OF REACTION SYSTEMS DIFFERING IN NEUROPHYSIOLOGICAL COMPLEXITY

A, with reactions of small amplitude as compared with those of Group B, indicates that the correlation may be one between amplitude and latency. Larger differences in latency were found between the normal and inhibited reflexes to light. The latency of 129 normal reflexes of Group B averaged 95.3 σ (*S.D.* 10.9 σ), while 40 inhibited reflexes whose latency could be measured averaged 106.4 σ (*S.D.* 18.8 σ). Difficulty in reading the records of responses of small amplitude tends toward increase in latency readings for these responses, and caution is necessary in interpreting obtained differences. Allison (1) found no latency differences for reflexes separated by intervals of 500 σ or more, although he found decided changes in voluntary reaction-time after stimulation at the same intervals. The alteration of reaction-time to a second stimulus occurs whether or not the first stimulus is reacted to, as Todd (34) and Woodrow (36) have shown.

The fact that reinforcement gives way to inhibition with increased time interval between successively presented stimuli has been known for a long time. Figure 5 presents for comparison certain examples from the literature of curves similar to those obtained for eyelid reflexes to light and sound.

The curve constructed after Erlanger and Blair (17) is a simple analogue of the more complicated processes. It shows the electrical response of nerve fiber (sciatic nerve, *Rana pipiens*) to induction shocks separated by brief intervals of time. This is a refractory phase curve in the strict sense of the term.

The curves of Eccles and Sherrington (15) from the flexor reflex of cats (decerebrate preparations with spinal transection) show results comparable to those of the present study. They divide their results into three types, those showing little inhibition, those showing some inhibition, but recovery by 50 σ, and those showing initial facilitation giving way to inhibition enduring beyond 80 σ. The latter is the most common, found in half of their 40 preparations, and it is the type agreeing most closely with the lid reflex results.

The curves from Yerkes' (37) experiments with the frog are important because he used intact animals and natural auditory and tactual stimuli. No leg response was given to the sound, but when the sound occurred before the tactual stimulus, the response to the latter was reinforced or inhibited, depending on the time interval. The whole process was spread out in time by using a prolonged auditory stimulus (electric bell) instead of the sound of a single tap of a hammer. Both curves are given.[7] This alteration under different stimulating conditions is an interesting experimental lead to be tried out on human subjects. To my knowledge it has not been done.

The data for the curve from Bowditch and Warren (2) were secured in experiments on the knee-jerk in which clenching of the hand followed a bell signal at certain intervals before a tap on the tendon. The reinforcing-inhibiting effect depended, as in all these examples, on the time interval between stimuli. The instructions to react to the signal means that many processes were actively under way at the time of the knee-jerk, and the spread-out effect may be similar to that which Yerkes found when he used a prolonged antecedent stimulus.

Hofbauer (24) carries the story of reinforcement and inhibition in relation to time interval into the field of voluntary contraction, and the curve (modified from Yerkes' plotting of Hofbauer's data) shows how interruption with a pistol shot affects the response of a muscle group which has been rhythmically contracting to the sound of a metronome.

The refractory phase studies of Telford (32) and Allison (1) for voluntary and associative processes fit the same general picture, although their units make comparison difficult. Neither used intervals below 500 σ, and their curves begin at the point of greatest inhibition. Recovery from inhibition with greater interval between stimuli proceeds to a supernormal phase. Supernormal phase has no representation in the curves of Figure 5, although it is suggested by some of the curves from the subjects of Group A (Figure 3).

The reinforcement-inhibition results of the present study are illustrative of response modification in relation to interval between stimuli found at many levels of neurophysiological complexity.

VI. Discussion and Interpretation

1. *Conditions Antecedent to Action.* Faint stimuli playing on the organism may be facilitating or inhibiting in an amount disproportionate to their effect when presented alone. The importance which Dodge (12) attaches to faint stimuli because of experiments involving repetitive stimuli of the same sensory modality become

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[7] Yerkes in 1906 brought together three of his earlier studies and added further data on the reactions of the frog (38). These studies have peculiar historical interest in relation to the conditioned reflex.

more significant when it is shown that the inhibitory stimulus may be of another sensory modality. The situation in which the interacting effects of two stimuli are studied reduces to simple form the very complex situation involved when postural adjustments or neuromuscular set furnish a background of behavior into which a stimulus is received. Reaction to a stimulus is always modified by these anticipatory adjustments; experiments with isolated stimuli may reveal the factors giving direction to such modifications. Parallels may be drawn also to experiments on the effect of distraction or incentive, in which are furnished stimuli supplementary to those of the responses studied. Action is seldom, if ever, the function of a single stimulus.

2. *Physiological Simultaneity.* It has been shown that reinforcement is greatest when response tendencies virtually coincide in time. This coincidence depends on the separation of the stimuli by a period of time equal to the difference in the latencies of the respective reactions. The difference in latency between the reflex reactions to light and sound (c. 50 σ) has a counterpart in the differences in voluntary reaction-time, and also in the estimation of time intervals involving light and sound. Ladd and Woodworth (26, p. 475) quote Exner's finding that, in order to tell which comes first, light must precede sound by 160 σ, while sound needs to precede light by but 60 σ. This discrepancy may be accounted for in terms of a 50-σ difference in the latencies of whatever processes are at the basis of discrimination of light and sound. (The difference in latency is added to the time required for discrimination when light precedes, and subtracted when sound precedes, hence the difference between 160 σ and 60 σ is twice the difference in latency of the two processes. Corrected for this 50-σ latency difference, the time required for discrimination is 110 σ regardless of which stimulus precedes.)

If consciousness of time in some way depends on our responses, the fact that reinforcement occurs at approximately physiological simultaneity, and is yet a process spread out in time, suggests that this 'enduring simultaneity' may be one of the factors involved in the 'specious present.'

3. *Physiology, Psychology, Refractory Phase.* The work of physiologists on lower forms is best interpreted for psychology by securing experimental parallels which call as little as possible for arguments by analogy. The parallels should ideally include data which suggest identity of process. Experimentation must continue

in fields differing widely in the complexity of phenomena treated, but only as the story of action systems is completed for both intact human subjects and animals can we get a physiological basis for understanding the more complex forms of human behavior. In the experiments reported, results from human subjects corresponded so closely to results obtained by physiologists on simpler preparations that one is tempted to postulate like factors underlying the behavior.[8]

In view of the physiologists' distinction between the relatively brief refractory phase of responding units and more prolonged periods of inhibition, the use of the term refractory phase for long-enduring states should be somewhat guarded. The avoidance of repetitive responses in situations such as those described by Thorndike (33), Telford (32), Luh (27), Allison (1) bears more resemblance to the long-lasting inhibitions than to the refractory periods from the long-enduring states are to be distinguished. In the broad meaning of a period of decreased sensitivity following stimulation, there can be no serious objection to the use of the word refractory, except that, for the physiologists, the term is now somewhat more restricted in meaning.

4. *Relation to Conditioning.* Conditioned reactions to light can be formed by repetitive presentation of the light preceding the sound at intervals of 200 σ to 400 σ, intervals at which the light exerts a depressing effect on the reflex to sound (23). The conditioned reaction occurs as a second reaction to light, anticipatory to the reflex to sound. It appears readily in the period between the two reflexes—a period in which reflex responses are greatly depressed; this fact emphasizes the physiological differences between conditioned reactions and the reflexes here being studied. The functional relationships between facilitation, inhibition, and the tendency for conditioned reactions to appear, have not been studied extensively; evidence to date suggests that the facilitory intervals are not favorable for conditioning (23).

5. *Clinical Applications.* The individual differences revealed in the reactions of the subjects of Groups A and B were striking. While chance selection in a small group might account for the "type" difference suggested, the pronounced and persistent differences

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[8] In order to secure similar samples of behavior from a lower form, experiments now under way with the cooperation of D. G. Marquis are exploring the eyelid reactions of normal and operated dogs under the conditions of the experiments here reported with human subjects.

suggest correlation of other factors, such as temperament, emotionality, intelligence. This applies also to the differences revealed in the reinforcement-inhibition curves. Thus far these possibilities are unexplored.

The uniformity of results with respect to intervals of reinforcement and inhibition makes possible the use of the method for detecting reflex lid responses to light, even when the light alone evokes no overt response. The clinical use of the reinforcement-inhibition method for detecting lid reactions to light would be similar to the use of the Jendrassik *handgriff* for facilitating the knee-jerk. Retinal sensitivity might be demonstrated in cases of functional or organic blindness when the reflex centers are intact, through presenting the light at an interval of 50 σ before the sound. For all subjects tested this interval has resulted in reinforced reaction to the sound. No exceptions have been found for five additional subjects tested at this interval, and to sound alone. All showed reinforcement of the reflex to sound by the preceding light. If the reflex to sound alone is of large amplitude, an inhibitory interval could be used, say with light preceding sound by 125 σ. By the use of one reinforcing interval, one inhibiting interval, and controls to sound alone, tendencies to react to light would be revealed, even though below the threshold for overt response.[9]

6. *Some Quantitative Problems.* The relation of the inhibitory power of a stimulus to its intensity requires study. This might give an objective analogy to Heymans' Law just as the variation of reflex response with the intensity of the stimulus corresponds closely to Weber's Law (29).

The light evoked some lid response from most of the subjects, but this may not be a necessary condition for obtaining reinforcement and inhibition. The use of a facilitating or inhibiting stimulus more remotely related to the responding member than the light in this experiment should give important information.

The effect of prolonged or repetitive stimuli (buzzer, flickering light) may reveal differences in response from that to momentary stimuli, as in Yerkes' experiments with the frog.

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[9] A case of hemianopsia is now being studied by this method with the collaboration of G. R. Wendt.

VII. Summary

1. Reinforced and inhibited reflexes to light and sound stimuli from ten human subjects were studied by the photographic recording method of Dodge.

2. Normal reflex responses to the light were of much smaller amplitude than normal reactions to the sound. When the light preceded sound by intervals of 25 σ and 50 σ, the reflex to sound was augmented by an amount greater than that of the normal reflex to light. At intervals of 75 σ and above the reflex to sound was inhibited, inhibition reaching a maximum when light preceded by 100 σ to 200 σ, and continuing at lessened degrees until 450 σ, the longest interval tested.

3. The interval of maximum reinforcement is such that the two reaction tendencies are approximately simultaneous, although facilitation continues when the reaction to light occurs anywhere in the contracting phase of the lid response to sound.

4. Inhibition sets in when the reaction to sound begins late in the contracting phase of the reaction to light, and continues for the longest interval explored. The results are compared with previous studies on animals and man.

5. The reflex to light is inhibited when sound precedes light. Inhibition shows in lengthening of latent period as well as in reduction in amplitude of response.

6. The results are discussed in relation to the conditions antecedent to action, the effect of faint stimuli, the conditioning process, clinical applications, and suggested quantitative problems.

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LE RENFORCEMENT ET L'INHIBITION DES RÉFLEXES DE LA PAUPIÈRE

(Résumé)

On a étudié les réflexes renforcés et inhibés de la paupière aux stimuli de lumière et de son chez dix sujets humains par la méthode d'enregistrement photographique de Dodge. Les réponses réflexes normales à la lumière ont été d'une beaucoup plus petite amplitude que les réactions normales au son. Quand la lumière a précédé le son à intervalles de 25 σ et de 50 σ, le réflexe au son a été augmenté d'une quantité plus grande que celle du réflexe normal à la lumière. A intervalles de 75 σ et plus, le réflexe au son a été beaucoup inhibé, l'inhibition arrivant à un maximum entre 100 σ et 200 σ, et continuant à plus petits degrés jusqu'à 450 σ, le plus long intervalle testé. On discute les conditions qui favorisent le renforcement et l'inhibition à la lumière des expériences physiologiques. On considère la signification de telles expériences pour la psychologie.

HILGARD

VERSTÄRKUNG UND HEMMUNG DER AUGENLIDREFLEXE

(Referat)

Verstärkte (reinforced) und gehemmte (inhibited) reflexive Reaktionen des Augenlides auf Licht und auf auditive Reize (sound stimuli) wurden an zehn menschlichen Versuchspersonen mit der photographischen Registrierungsmethode von Dodge untersucht. Die normalen reflexiven Reaktionen auf das Licht hatten einen viel kleineren Umfang (amplitude) als normale Reaktionen auf den Laut (sound). Wenn das Licht dem Laut mit einem Intervall von 25 σ und 50 σ vorausging, wurde die reflexive Reaktion auf einen auditiven Reiz (sound) verstärkt durch einen Betrag, der grösser war, als der normalen reflexiven Reaktion auf Licht. Bei Intervallen von 75 σ oder mehr, war die reflexive Reaktion auf auditive Reize stark gehemmt. Die Hemmung erreichte zwischen 100 σ und 200 σ seinen Höhepunkt, und dauerte dann in abnehmender Stärke bis zu 450 σ, dem längsten geprüften Intervall, fort. Die Einwirkungen, die die Verstärkung und die Hemmung begünstigen, werden im Lichte physiologischer Experimente besprochen. Es wird die Bedeutung solcher Experimente für die Psychologie erwägt.

HILGARD

ON THE RATE OF EXTINCTION OF A CONDITIONED REFLEX*

From the Laboratory of General Physiology of Harvard University

B. F. Skinner

I

A method of investigating the rate of formation of a conditioned reflex has been described in a previous paper (1). Without any essential modification it is suitable also for the study of experimental extinction, which differs from conditioning only in the direction of the observed change. In conditioning, an originally weak "investigatory" reflex, arbitrarily chosen for the experiment, is strengthened when the elicitation of the response is followed immediately by a second stimulus leading to the ingestion of food. In extinction, on the other hand, the response, once conditioned, is then not followed by the second stimulus, and the strength of the reflex diminishes. Thus conditioning appears experimentally as an increase in reflex strength, extinction as a decrease. In both cases the rate at which the response is elicited under constant stimulation may be used as a measure of the strength. The process of extinction is investigated in the following way.

The general procedure has been described in reporting the experiments on conditioning (1). The apparatus consists, in part, of a food-magazine, a food-tray, and a lever, so arranged that when the lever is pressed downward (by a rat) the magazine discharges a small pellet of a standard food into the tray. Four sets of the apparatus are operated simultaneously. In investigating extinction the response to the lever is first maximally conditioned. The connection between the magazine and lever is then broken, and the subsequent responses of the rat are recorded in the usual manner.[1] The accom-

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[1] As an alternative method the magazine may be left functional but empty. The response to the lever is then followed as usual by the click of the magazine. Responding to the click, the rat approaches the food-tray, but the stimulation ordinarily supplied by the pellet of food is lacking, and the chain is broken at that point. Although this is a relatively complex case, it seems to involve no significant difference in the rate or in the change in rate of extinction. A minor difference in the behavior is mentioned in Footnote 5.

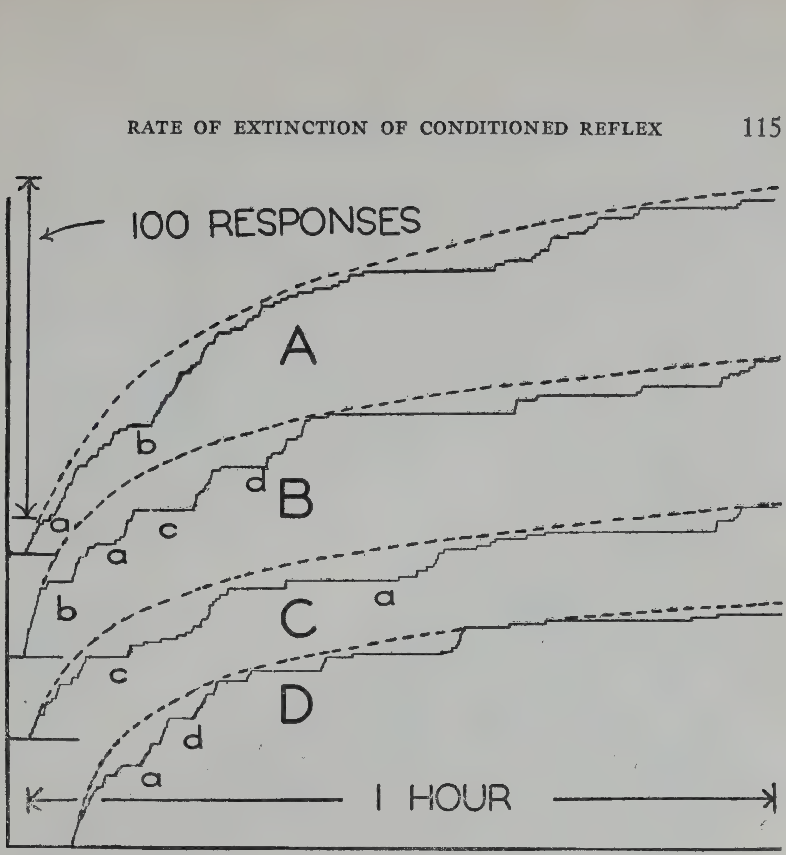

FIGURE 1
Four Curves for Original Extinction
See text.

panying figures are reproduced from kymograph records, to which coordinates have been added. Each response is represented by a short (uniform) elevation in the recorded curve. The rate of elicitation is given by the slope, and the records are, in effect, number-time graphs of the responses to the lever. As a necessary modification in dealing with the higher rates observed in extinction, the kymographs are here moving twice as fast as in the experiments reported in (1).

II

Four male rats, members of the 18th generation of an inbred strain,[2] 98 days old, were conditioned in the manner previously described. Records of the conditioning of this group will appear in a later communication. At the time of the conditioning the rats were permitted to obtain about 100 pellets of food, pressing the lever once for each pellet. They were then removed from the apparatus, and extra food was given them for one hour. On the following day the magazines were disconnected, so that the movement of the lever was without effect, except upon the recording devices in an adjoining room. The rats were placed in the experimental boxes and released at the usual time and in the usual way. Their responses to the lever during the following hour are recorded in Figure 1. The animals were released approximately at the beginning of the curves, except Rat D, which showed a characteristic delay in going through the one-way door of the release compartment, and whose record begins about five minutes after the release.

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[2] Known in this laboratory as the *P* strain. For its genetic history see Crozier, W. J., and Pincus, G., *J. Gen. Physiol.*, 1931, **15**, 243.

When the first response to the lever fails to supply the stimulus for the next member of the usual chain, the response is elicited again immediately, and a high rate of elicitation is maintained for a short time. It is evident from the figure, however, that this is soon interrupted and that the rate subsequently undergoes an extensive fluctuation. In spite of the irregularity it is possible to indicate a general course for each curve. When this is done, as in Figure 1, it is apparent that the deviations have the character of depressions in the curve, and that they are followed by compensatory increases in rate. This point is discussed in detail later. Because of the compensation the smooth curve is eventually fairly closely approximated. The theoretical curves in Figure 1 are logarithmic. Their agreement with the experimental curves is also discussed later.

At the end of the experimental period represented in Figure 1 a low rate of elicitation has been reached. The strength of the reflex has been reduced approximately to its value prior to conditioning. But if the rats are returned to the apparatus on the following day (no reconditioning having taken place), small extinction curves are obtained. A "loss of extinction" has occurred in the intervening 23 hours. Such an effect ultimately disappears if the extinction is sufficiently prolonged, and it is probable that a state of complete extinction can be reached. Since the reflex may persist, however, as an investigatory reflex in spite of the extinction, we are limited to the above statement that the frequency of elicitation has been reduced approximately to its value prior to conditioning.

When any degree of extinction has been attained, the reflex may

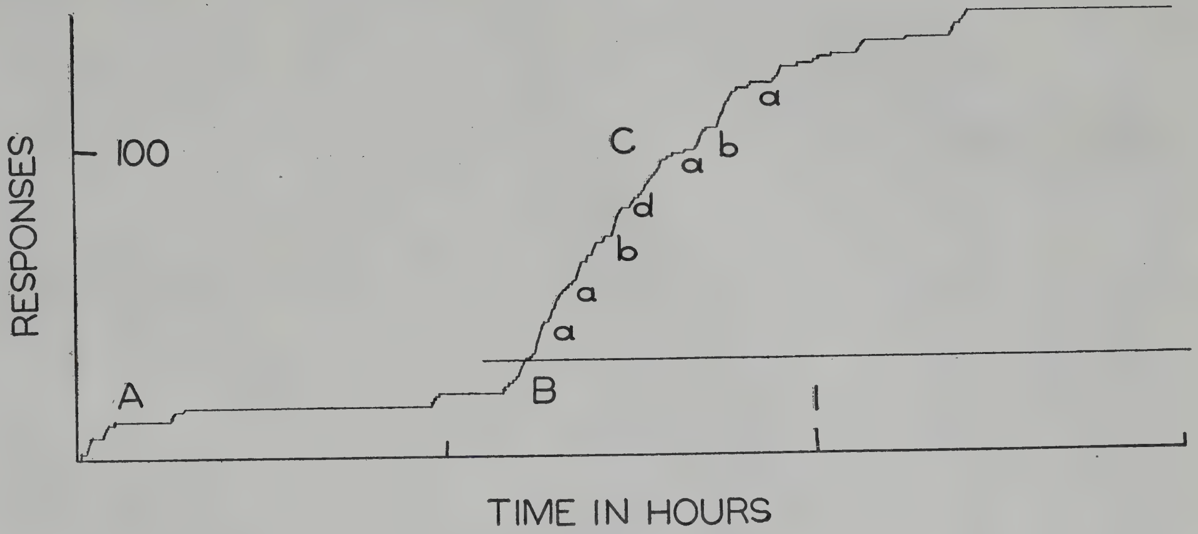

FIGURE 2

Record of Reconditioning and Subsequent Extinction

At *A:* some loss of the very thorough extinction obtained on the preceding day. At *B:* reconditioning as ten pellets of food are obtained. At *C:* curve for extinction following reconditioning.

See text.

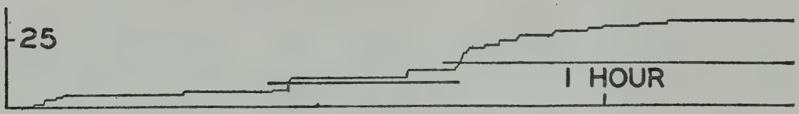

FIGURE 3
See text.

be restored to full strength by reconditioning. It may then be extinguished again in the same way, subject to the modification noted below. In Figure 2, taken with another rat, the reflex had been thoroughly extinguished on the preceding day, with the alternative method described in Footnote 1. Some "loss of extinction" is shown in the small curve at *A*. The rate then reached a very low value. At *B* 10 pellets of food were put into the magazine, and these were obtained by the rat with the 10 responses shown at that point.[3] The resulting extinction follows at *C*. A base-line for the curve for extinction has been added. The details of the curve are discussed later.

That such an increase in strength is actually a reconditioning and not simply a facilitative effect due to the reception of food is shown in Figure 3. In this experiment the reflex had been very thoroughly extinguished. There is a slight loss of extinction evident at the beginning of the curve, followed by only two responses during the next 25 minutes. Three pellets of food were then discharged from the magazine by the experimenter, the lever remaining disconnected. The rat pressed the lever only five times in the following 18 minutes, indicating that a facilitative effect, if present, was very slight. At the next response the magazine had been connected, and the rat obtained three pellets of food, pressing the lever once for each. The magazine was then disconnected and a significant curve for extinction obtained.

At practically complete extinction the strength of the reflex is equal in value to its strength prior to conditioning, and there is no observable modification in the behavior of the rat. The process of extinction has thus simply reversed the change observed in conditioning. Nevertheless, the combined process of conditioning-and-extinction has left a detectable effect upon the organism, which appears experimentally, not as a change in immediate behavior, but as a modification in the rate or in the rate of change of the rate of some subsequent process. When, for example, a reflex is reconditioned and then extinguished a second time (the two amounts of conditioning being roughly equal), the area under the second curve for extinction is reduced. The strength of the reflex, measured at the beginning of the two processes, may be the same, but the change in strength proceeds much more rapidly in the second case. Thus there is no simple relation between the strength of the reflex at a given moment and what might be called its resistance to extinction beyond that moment.

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[3] The acceleration exhibited in the curve for reconditioning is not always obtained under apparently comparable conditions. If it is real, it may represent an inhibitory effect of the preceding extinction, but the point cannot be settled on the existing evidence.

A similar effect, an example of which may be mentioned for its bearing upon extinction, is commonly known as over-conditioning. We have seen elsewhere that when the response to the lever is originally conditioned the reflex may reach an essentially maximal strength upon only one occasion when its elicitation is followed by the discharge of food from the magazine (1). It is probable, however, that a curve for extinction of the magnitude of those shown in Figure 1 could not be obtained after only one elicitation of this sort. On the other hand, curves of greater magnitude might have been obtained in Figure 1 if the number of responses when food was obtained had been greater. Although there may be no significant increase in the immediately observed strength of the reflex after the first (conditioning) elicitation, there is possibly an extensive increase in resistance to extinction. Nevertheless, a high degree of resistance may be reached in a very short time, as shown in Figure 4. Here the first five responses indicate the practically instantaneous conditioning of the reflex. The initial response was the first in the history of the rat to be followed by the delivery of food. Although there was a slight delay after the second response, the average rate for the first five is very near the maximal for a normal eating curve.[4] After the fifth response the magazine was disconnected and the reflex extinguished as shown. Although conditioning and over-conditioning were possible at only five elicitations, the curve for extinction is composed of more than two hundred responses, and the rate has not then closely approached zero. The details of the curve, especially the two major breaks at *A* and *B*, are discussed later.

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[4] The record thus confirms the observations reported in (1). In comparing the records the difference in the time coordinates should be noted.

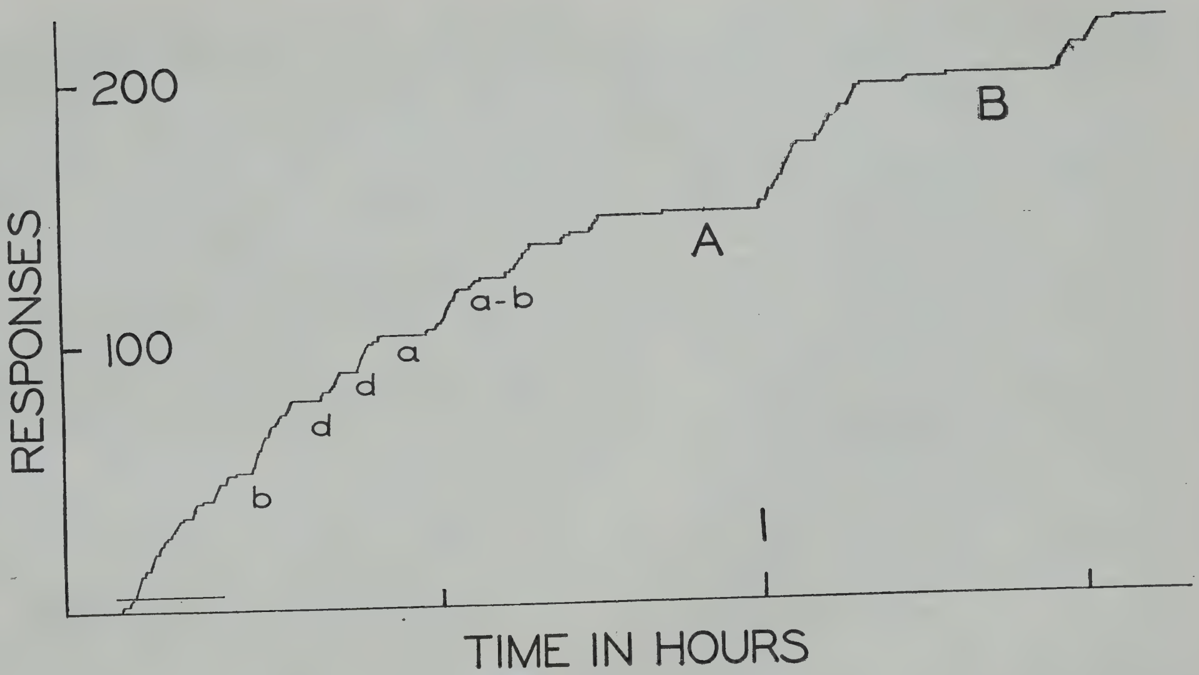

FIGURE 4

The beginning of this record shows the original conditioning of the reflex. The first five responses (as far as the horizontal mark) are the first and only responses to the lever to be followed by the delivery of food. The subsequent extinction is shown in the rest of the record. The breaks at *A* and *B* are due to conflicting stimuli inadvertently introduced into the experimental box.

What we have called resistance to extinction is properly beyond the scope of this paper. It belongs to a class of effects which have so far failed to receive an adequate experimental treatment. They present a special problem, because, as we have noted, they appear as modifications, not in immediate behavior, but in the rate of change of the rate of some process. A quantitative investigation of "resistance to extinction" would presuppose some method of measuring the course of the extinction itself and some elementary knowledge of its nature. We are concerned here with these preliminary steps.

III

Sixteen records of original extinction and several hundred of extinction-after-reconditioning have been examined. None of them calls for any significant modification of the statements in the preceding section. The specimens reproduced are typical. In most cases better approximations to a smooth curve could have been given, but they would have represented the average result less accurately. In general, the curves show marked deviations; and this must be accepted as characteristic of the change in reflex strength observed under these conditions. The deviations are not, however, haphazard. The intervals elapsing between successive responses are distributed by no means at random, and the observed fluctuations are, in that sense, real. Before giving some account of their nature, it will be well to consider several possible sources of disturbance in the recorded curves and certain incidental changes in the behavior of the rat, which must be taken into account in determining the course of the major process.

There is, first of all, the possibility, elsewhere discussed, that the recording apparatus may fail to follow the behavior of the rat. The normal eating curve is regular enough to expose errors of this sort, but records obtained with ordinary care seem to be free of them. In the process of extinction, however, much higher rates must be dealt with. A greater strain is placed upon the apparatus and particularly upon the device introduced to meet the chief difficulty of the method, which arises from the use of an electrical contact in recording the displacement of the lever (3, 4). To check the accuracy of the apparatus a special signal has been used which operates independently of the recording device and enables the experimenter to compare the record with the actual make-and-break in the circuit. The present records are believed to be correct reports of the movement of the lever.

Secondly, it is possible that the *character* of the response may change during extinction, in such a way that the closing of the mercury switch at the lever will no longer serve as a reliable criterion of the behavior. But this is evidently not true. Throughout the course of the extinction the contacts are, in general, cleanly made and broken, and their duration holds rather closely to a constant average value.[5]

A third possible source of irregularity is the base line upon which the process of extinction is observed. An unconditioned rat will occasionally respond to the lever. A certain number of these "investigatory" responses will be recorded as the inevitable result of the activity of the rat within the narrow limits of the experimental box. The rate of elicitation is irregular and varies with the hunger of the animal. Responses of this sort offer no difficulty in recording a normal eating curve, because the ingestive reflexes are prepotent during the early part of the curve and the investigatory reflexes are weak during the latter. In the process of extinction, however, the rate of elicitation approaches, not zero, but a value characteristic for investigatory reflexes in a *hungry* rat. This is a real, if not highly important, factor. It means in practice that we may expect to find a fairly small number of responses, distributed at random throughout the curve, that appear as deviations from an otherwise rational course. [It is well to make a special class of those instances in which the displacement of the lever is the result, not of a response, but of the movement of the rat under the influence of mechanical forces. Such would be the case, for example, if the rat fell against the lever. Compare (2, p. 444).]

We must also consider certain incidental deviations from a smooth curve, correctly reported by the apparatus, which reflect genuine changes in the behavior of the rat. We have discussed an example elsewhere, in dealing with the modification in reflex strength observed during the ingestion of food (3, 4). In the normal eating behavior of the rat short periods frequently occur in which no eating takes place. They are followed typically by compensatory increases in rate, so that only temporary divergences from the normal eating curve result. It is easily shown that such an effect is not part of the process represented by the main curve. The delays occur haphazardly during an experimental period, and they may be absent altogether. They are more frequently observed when the reflex is relatively weak, and this suggests that they are due to the operation of conflicting, momentarily prepotent stimuli.

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[5] In the special case in which the magazine is functional but empty (see Footnote 1), the second reflex in the chain is extinguished first, and the initial response is then not terminated as quickly as usual. In such a case the lever may be held down at a single response for as long as thirty seconds. As already noted, no significant difference in rate or in change in rate has been observed between the two procedures.

The same effect is observed when the response is not followed by the ingestion of food, as in the process of extinction. In the present curves, however, most of the examples are obscured by the rapid fluctuation in rate, into which, as we shall see shortly, the delays and recoveries have become incorporated. Since the effect is used in the following analysis, some independent evidence is called for. Fortunately, curves for extinction can be obtained that are not subject to the characteristic fluctuation observed with the present method. The necessary procedure will be described in a later communication, where examples of the effect in question will be given. A deviation will also be demonstrated, of a converse sort, in which a brief increase in rate is subsequently compensated for by a period of delay. The existence of both effects may for the present simply be assumed.[6]

IV

From a casual inspection of Figures 1, 2, and 4, it is apparent that the typical curve for extinction is wave-like in character. There is no very uniform wave-length or amplitude, but the rate of elicitation clearly tends to pass from a high to a low value and back again. The transitions are sometimes smoothly executed but are more often abrupt. Since an effect of this sort is presumably continuous, we may regard the smooth transition as representative of the actual change in strength, and we may then account for the abrupt transitions by appealing to some of the disturbing factors discussed in the foregoing section.

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[6] An example of the effect of extraneous stimuli was obtained inadvertently in the experiment recorded in Figure 4, where a short piece of heavy twine was left projecting into the box and was chewed to bits by the rat during the course of the experiment. This must have been accomplished during the two major breaks at *A* and *B*, for here only could the rat have found the necessary time. The breaks are unusual (considering the slope at which they appear) and call for some such special explanation. Granted the present one, the record offers an excellent example of the effect of conflicting stimuli and of the compensating process of recovery.

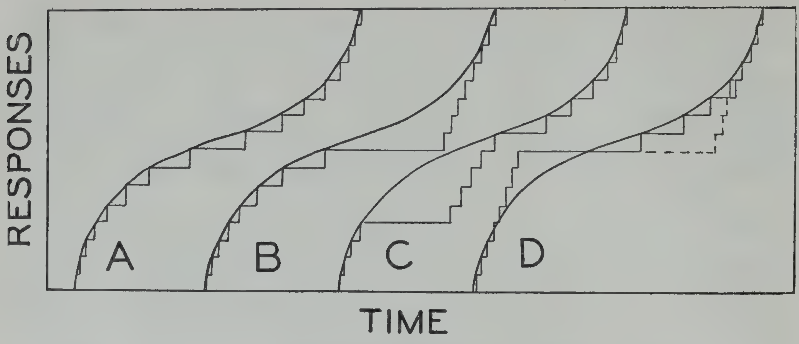

FIGURE 5

Schematic Representation of a Possible Approximation to a Smooth Curve and Some of the Effects of Minor Deviations

See text.

If we assume a continuous change in strength as given by the curved lines in Figure 5, then the best possible representation with the present method is shown at *A* in that figure. The weak part of such a curve is the middle section, where the strength is low, and if a break is observed, it will probably be in that region. A delay of the sort just described, together with its subsequent recovery, would affect the record approximately as shown at *B*. The deviation may appear at some other part of the curve, however, and the result will then be otherwise, for example, as at *C*. The converse type of deviation will give other characteristic effects. If the acceleration appears, as it most often does, when the rate is passing from a high to a low value, the theoretical curve will be overshot as shown at *D*, and the latter half of the cycle may then be of either the *A* or the *B* type.

We have used for illustration a complete cycle composed of 16 responses and have assumed an incidental deviation affecting about one fourth that number. The ease with which a deviation can be identified and allowed for will depend, of course, upon this ratio. Consequently, it is important to notice that both the wave-length and the magnitude of the deviation may vary widely from these selected values. Furthermore we have assumed a simple and quite arbitrary wave-form. A quantitative treatment of the fluctuation would require a closer reference to the actual data than this elementary scheme possesses. Our present purpose, however, is to

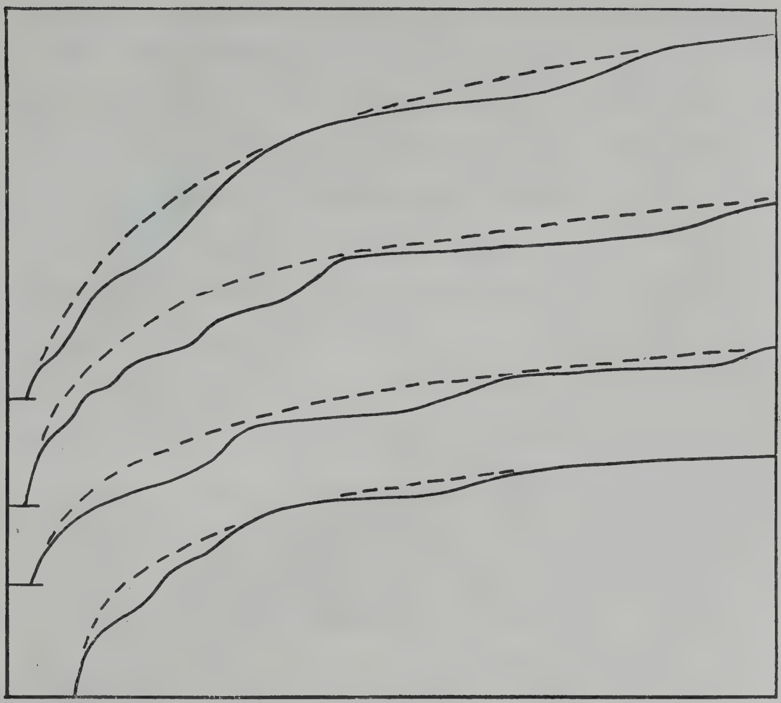

FIGURE 6

Inferred Course of the Changes Recorded in Figure 1, Allowance Having Been Made for Expected Deviations

See text.

show simply that the curves are wave-like in nature; and this can be done qualitatively by pointing out that the records in Figures 1, 2, and 4 can, in general, be divided into cycles corresponding to one or another of the types in Figure 5. The principal examples in the former figures have been lettered (lower case) according to type. The effects are not always clear-cut, but we have seen that the curves are subject to other minor disturbances. The probable course of the change in strength in the four curves in Figure 1, as distinct from the disturbed course given experimentally, may be represented as in Figure 6.

A simple theoretical account of this cyclic fluctuation can be derived from the assumption that an interruption of the normal chain of ingestive reflexes sets up a depressive effect that lowers the rate of elicitation. In order to account for the present observations we must assume a time-lag for the effect and must appeal also to a previously demonstrated compensatory increase in rate following an incidental delay (3). In the process of extinction the rate of elicitation begins at a maximum. Under the conditions of the experiment each elicited chain is interrupted when the necessary stimuli for the continuance of the chain are not supplied, and, by assumption, a depressive effect is generated. A period of reduced rate supervenes, during which the previously generated effect disappears and (because of the lowered rate) is not replaced to any considerable extent. Through a compensatory increase the rate returns to a maximal value, where it enters upon a second cycle. The properties of the resulting curve will depend upon the extent of the depressing effect of a single interrupted elicitation, upon the time-lag, and upon the nature of the process of recovery.

Such a depressive effect is traditionally described as "emotional." It appears experimentally simply as a decrease in reflex strength, and may be defined as such. One characteristic is that it readily adapts out. As we have noted above, curves for extinction may be obtained that lack this cyclic effect. The essential condition is simply that time be provided for adaptation.

In Figure 6 we may inspect more easily the theoretical curves that have been fitted to the data. They are drawn as envelopes, upon the general assumption that the deviations are depressions. The above account of the fluctuation is based, of course, upon that assumption, since it requires the existence of a compensatory effect. Furthermore, the first short section is heavily weighted in each record, and the theoretical curves are permitted to lie considerably above the immediately following sections of the curves. This is also justified by the present interpretation of the fluctuation. Since we have assumed a time-lag, the first part of the curve must be free of the depressing effect. Moreover there is no reason to expect the first few recoveries from depression to be successful in reaching the extrapolated curve, since the generation of a depressing effect is in no way related to whether or not the rat is "on schedule." *Eventually* the theoretical curve is reached, because the depressive effect partially adapts out, and because the curve has meanwhile fallen off.

We can summarize the present analysis by listing the assumptions it involves:

1. The decrease in reflex strength observed during the process of extinction follows an approximately logarithmic course.
2. The essential condition for extinction (the interruption of the chain) leads also to a temporary ("emotional") change, which depresses the rate of elicitation.
3. The depressive effect shows a time-lag, which, in conjunction with a previously demonstrated compensatory increase in rate, produces a cyclic fluctuation.
4. The depressive effect is subject to adaptation.

These are all apparently reasonable assumptions, and they are the simplest set capable of accounting for the recorded curves. They are susceptible of independent investigation, and it should eventually be possible to give a complete quantitative description of the type of curve here reproduced.

SUMMARY

A previously described method of investigating the formation of a conditioned reflex is suitable also for the study of experimental extinction. A procedure is described and four records are reproduced. The typical curve can be fitted with a logarithmic curve drawn as an envelope. The experimental curve, however, shows a characteristic fluctuation.

Several minor types of disturbance may be present in records of this sort. When due allowance has been made, the resulting inferred curve is wave-like in nature. The fluctuation is cyclic. It can be accounted for by assuming that the essential condition for extinction (the interruption of a sequence of reflexes leading, in this case, to the ingestion of food) generates a temporary "emotional" effect that depresses the rate of elicitation.

Curves are also reproduced showing extinction after reconditioning and extinction immediately after the original conditioning. In the latter case a very large ratio is observed between the number of times the reflex is elicited during extinction and the number of elicitations at which conditioning or over-conditioning could have been possible. It is also demonstrated that the increase in reflex strength observed in reconditioning is not simply a facilitative effect due to the consumption of the food obtained.

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SUR LA VITESSE DE L'EXTINCTION D'UN REFLEX CONDITIONNEL

(Résumé)

Une méthode déjà décrite pour l'investigation de la formation d'un réflexe conditionnel est aussi bonne pour l'étude de l'extinction expérimentale. On décrit un processus et reproduit quatre notations. La courbe typique peut être représentée par une courbe logarithmique dessinée comme enveloppe. La courbe expérimentale montre cependant une fluctuation caractéristique.

Plusieurs types moins importants de confusion peuvent être présents dans des notations de cette sorte. Quand on s'en est rendu compte, la courbe inférée qui résulte ressemble aux ondes en nature. La fluctuation est cyclique. On peut l'expliquer en supposant que la condition essentielle de l'extinction (l'interruption d'une séquence de réflexes menant dans ce cas à l'ingestion de la nourriture) génère un effet "émotif" temporaire qui déprime la vitesse des réponses.

On reproduit aussi des courbes qui montre l'extinction après un nouveau conditionnement et l'extinction immédiatement après le premier conditionnement. Dans le dernier cas on observe un très grand rapport entre le nombre de fois que le réflexe se présente pendant l'extinction et le nombre de réponses où le conditionnement ou trop de conditionnement auraient pu être possibles. On montre aussi que l'augmentation de la force du réflexe observée dans le nouveau conditionnement n'est pas simplement un effet facilitant dû à l'action de manger la nourriture obtenue.

Skinner

ÜBER DIE SCHNELLIGKEIT DES AUSSTERBENS (RATE OF EXTINCTION) EINES BEDINGTEN REFLEXES

(Referat)

Das schon beschriebene Verfahren zur Untersuchung der Gründung (formation) eines bedingten Reflexes lässt sich auch auf die Untersuchung der experimentellen Abschaffung anwenden. Es wird ein Verfahren beschrieben und es werden vier graphische Registrierungen (records) wiedergegeben. Der typischen Kurve lässt sich eine logarithmische Kurve anpassen, die als Umkreis (envelope) gezeichnet wird. Die experimentelle Kurve erweist aber ein karakteristische Schwankung (fluctuation).

Es können sich in Registrierungen dieser Art mehrere Arten von geringeren Störungen zeigen. Werden diese Störungen in Betracht

gezogen, so ist die resultierende zu folgernde (inferred) Kurve wellenartig. Die Schwankung ist zyklisch. Man kann sie durch die Annahme erklären, dass die zur Abschaffung wesentliche Bedingung (die Unterbrechung einer in diesem Falle zu der Einnahme von Nahrung führende Reflexenreienfolge) (sequence of reflexes) eine affektive ("emotional") Einwirkung hat, die die Schnelligkeit der Hervorrufung hemmt.

Es werden auch Kurven wiedergegeben, die die Abschaffung nach Wiederbedingung (reconditioning) und die Abschaffung sofort nach der ursprünglichen Bedingung darstellen. Im letzteren Fall findet man eine sehr hohe Verhältnisszahl (ratio) zur Aüsserung der Beziehung zwischen der Zahl der Hervorrufungen (elicitations) des Reflexes während der Abschaffung und die Zahl der Hervorrufungen bei denen Bedingung oder Überbedingung (over-conditioning) hätten möglich sein können. Es wird ebenfalls erwiesen, dass die während der Wiederbedingung beobachtete Zunahme der Stärke des Reflexes nicht bloss ein fakultatives, durch die Einnahme der erhaltenen Nahrung verursachtes Ergebniss ist.

SKINNER

REFLEX AND VOLUNTARY REACTIONS OF THE EYELID*

From the Institute of Human Relations of Yale University[1]

HELEN PEAK

THE PROBLEM

A great deal has been written about the classical categories of reflex and voluntary behavior, much of which has been devoted to pointing out their inadequacy. Indeed, from some quarters there has been a tendency to abandon such distinctions altogether. This is particularly true of those who would make the simple reflex the prototype of all behavior and the term, "reflex," synonymous with any demonstrated correlation between stimulus and response.[2] Other observers have marked the continuity of behavior in the course of development and the difficulty of assigning a point at which a response ceases to be "voluntary" and becomes "reflex."[3] Yet others insist on the complexity and the intercorrelations found in any behavioral cross-section, a fact which necessarily makes difficult the classification of such cross-sections into categories. Dewey (3), for example, in his well-known article on the reflex arc emphasizes the the complication of interaction and coordination existing in every response situation, and Dodge (5) has shown more recently that a reaction to a given stimulus complex practically always involves successive phases, the results of various degrees of elaboration of the sensory data, so that it is inappropriate to call the complete response either reflex or voluntary.

Because of such observations, it is generally admitted that the dichotomising of behavior is not the unambiguous procedure that it

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[1] Grateful thanks are due Professor Raymond Dodge for reading the manuscript and for counsel and criticism at many points in the course of the investigation.

[2] In a recent article Skinner (22) sets forth this concept of a reflex and, like Pavlov, makes the term denote chiefly that behavior is determined.

[3] See Fearing's genetic group (6, pp. 289 ff. and 301 ff.). Wiersma (26) cites experimental material bearing this out.

was once thought to be by men like Marshall Hall. Behavior described in most dimensions is continuous. But, despite this fact, the very persistence of the distinction between reflex and voluntary action, even where the terms themselves have been abandoned, indicates that the summary denial of the distinction has failed to settle the issue. It is important, therefore, that the problem should receive a thorough-going investigation both experimentally and theoretically. In the present paper we shall report results of an experiment which provides the data for a comparison of descriptive features of reactions of one muscle group, the human eyelid. These results will indicate some of the limits within which reflex and voluntary responses may be identified by certain of their objective characteristics, as well as cases where the characteristics are indistinguishable. Since this procedure implies at the outset that we have somehow distinguished reflex and voluntary reactions, the terms are defined tentatively for the purposes of, and in terms of, the present experiment. The validity of such definitions and some of the more general problems involved in them will be considered at length in a second paper to be published elsewhere (14).

These tentative definitions rest on the following tendencies which may be demonstrated in the lid responses of cooperative and unpracticed subjects: (1) If a barely perceptible noise is presented when the subject has been instructed to relax, no overt response occurs. (2) When a noise of large intensity is given to a subject who has been told to relax, the response consists commonly of lid closure beginning at about 40 sigma after the stimulus. (3) When the barely perceptible noise is presented after instructions to wink voluntarily in response to this noise, we again obtain lid closure, but this time the response has a much longer latency, usually 90 sigma or more. (4) When the intense noise is given after instructions to wink voluntarily, two lid responses follow: the first at a latency of 40 sigma, the second usually at 90 sigma or more. These facts indicate that *under the conditions stated,* responses of two distinct latencies are obtained; that the presence of the first short latency response is dependent on a noise above a certain intensity; that the presence of the second response is relatively independent of the noise intensity but markedly dependent upon the instructions under which the subject is reacting. The first short latency response, dependent upon the presence of a noise above a given intensity, is called the reflex wink; the second, longer latency response, with a less marked dependence on the noise than on the instructions, is the voluntary wink.[4]

These definitions have followed the common practice of describing reflex and voluntary responses in terms of their latency, the characteristic which has been employed recently as a criterion in the determination of the reflex or voluntary origin of the conditioned response.[5] Since there are situations where latency cannot be determined, as, for example, where the end of the first reaction and the beginning of the second overlap (15, p. 17), or where the moment of occurrence of the stimulus is not known, it would clearly be of value here, as well as in cases where latency is available, if other characteristics could be found to serve as supplementary means of identifying responses. It is, therefore, the purpose of the present experiment to examine certain additional attributes of the lid reaction, the conditions of variation in these attributes, and the limits within which they may be said to characterize reflex or voluntary lid response.

The four features of the lid reaction which have been examined are (1) latency, (2) amplitude, (3) closing velocity, and (4) opening duration, the last characteristic having been investigated the most completely. The specific meaning of these features follows. (1) *Latency* is the time intervening between the stimulus and the onset of closing of the lid. (2) *Amplitude* is the distance in millimeters which the lid moves from the position of rest to the position of maximum closure. (3) *Closing velocity* is the number of millimeters which the lid closes during 10 sigma of time measured in the region of greatest velocity of closure. This particular measure of velocity is justified on the following grounds: (*a*) It shows a positive relation to opening duration. (*b*) Though obviously not the only possible measure of closing velocity, there is evidence that it may be a representative measure, for Isserlin has shown (11, p. 76) that when velocity is measured at each tenth-second interval of the curve of limb extension or reflexion the highest velocity rate bears a constant relation to the average velocity of the entire rising phase

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[4] It is not implied here that either response is independent of instructions or noise intensity. In the intact organism it seems likely that most conditions impinging upon the organism from within or without have their effect on all responses. This will be discussed in the second paper.

[5] Hamel (8), Cason (2), Humphrey (10), Schlosberg (17), Wendt (25), Hilgard (9).

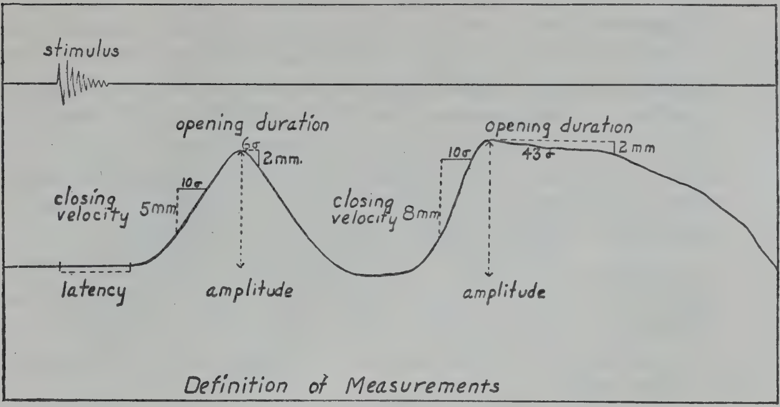

FIGURE 1

Definition of Measurements

The distances along the dotted lines are the values in which the four measures are stated: latency (in sigma); amplitude (in millimeters); closing velocity (millimeters in 10 sigma); and opening duration (time from maximum to 2 millimeters of opening). The measures are illustrated for a reflex and a voluntary response.

of the movement. (*c*) Reliability coefficients calculated from two independent readings of a group of unselected records of two subjects indicate that the selection of this region of greatest velocity can be made with accuracy. One hundred eighty-three records from subject Go show a coefficient of +0.974; 106 records from subject Wa, +0.977. (4) *Duration of opening* is the time elapsing from the point at which the lid reaches its first maximum closure to the point at which it has opened 2 millimeters above this. Reliability coefficients of this measure calculated from the same records indicated above are +0.94 and +0.95, respectively. This measurement was selected for the following reasons: (*a*) It is an absolute value, not related to another measurement such as amplitude. If measurement were made to some relative point such as half opening, then responses of large amplitude would have larger opening values simply because of the greater absolute distance which the lid must open in this case in order to reach half opening, so that, even though the absolute rate of opening were the same in a small and large response, the measure would indicate that the response of greatest amplitude had a longer opening time, because of the spurious correlation of opening duration with amplitude. (*b*) The measure of opening is stated as "duration" instead of "rate" because of the fact that, in voluntary response, the lid often remains closed for varying lengths of time before opening begins, in which case rate appears to be inappropriate. In the reflex, a rate measure is available if desired and would be appropriate, for the records show that there is rarely a plateau at the point of maximum closure and that opening begins promptly. It follows, therefore, that in the voluntary response the opening duration measure often includes delay in onset of opening as well as rate of opening once begun. (*c*) Duration or rate of opening, measured high up on the curve as has been done here (within the first 2 millimeters of opening), tends to rule out the effect of secondary responses which often appear as "humps" on the downward swing of the curve.

The Experiment

A noise stimulus of constant intensity was used throughout the experiment while the instructions were varied in order to determine their relation to the stated attributes of response in reflex and voluntary lid reactions, and to establish some of the limits within which these reactions are descriptively different. As we have indicated previously, the characteristic which has been called "opening duration" appeared to offer the most likely basis of distinction, and it is this characteristic with which the present experiment is most concerned. The instructions were of three kinds: (1) that which was neutral with regard to opening duration, (*a*) in one situation, the subject was asked to relax, (*b*) in another, he was instructed to wink voluntarily, no reference being made to opening duration; (2) that directing that the opening duration of the reflex be lengthened, i.e., made to resemble the voluntary; (3) that directing that the opening duration of the voluntary be decreased, i.e., made to approximate the reflex. The specific instructions follow:

Instructions Ia (relaxation)

1. "When you see the white light flash on at your side, look at the little red cross directly in front of you until after the noise has occurred.

2. "During this time make no effort of any kind. Relax as completely as possible and pay no attention either to the stimulus or to any reaction which you may or may not make to the stimulus. Just be sure that you are comfortable and relaxed."

Instructions Ib (voluntary winking—uninstructed duration)

1. First item of *Ia* repeated.

2. "At the same time get yourself ready to blink voluntarily as quickly as possible in response to the sound which you will hear in the headphones about two seconds after the light flashes. You may possibly wink reflexly first, but pay no attention to the presence or absence of the reflex. Just be sure that in the interval between the 'ready' signal and the stimulus you are attending to the movement of blinking.

3. "After you have reacted to the stimulus report to the experimenter whether your preparation for the reaction was 'good,' 'medium,' or 'poor.' Do not judge this in terms of the speed of your reaction but only on the basis of whether you made the maximum preparation during the pre-stimulus interval."

Instructions II (prolong reflex closure)

1. Same as *Ia.*

2. "A short time after you have fixated the light you will hear a noise in the headphones. This will cause the eyes to close and open reflexly. This is the reflex wink. You are to make an effort to avoid opening the eyes when they have once closed. Do not prevent their closure but keep them closed after they have reached that position."

3. Report instructions repeated.

Instructions III (wink voluntarily; open promptly)

1. Same as *Ia.*

2. "At the same time get yourself ready to blink voluntarily as quickly as possible in response to the sound which you will hear in the headphones about two seconds after the light flashes. Make an effort to open your eyes promptly after you have closed them voluntarily. You may possibly wink reflexly first but pay no attention to the presence or absence of the reflex. Just be sure that in the interval between the 'ready' signal and the stimulus you are attending to the movement of closing and opening your eyes as quickly as possible."

3. Report instructions repeated.

Six subjects, undergraduate men in Yale College, were used in the experiment. A series of lid responses was recorded from each subject under Instructions I*a* to serve as norms for the characteristics of the uninstructed reflex. In the following session two subjects continued under these same conditions, two under Instructions I*b*, and one each under II and III. This constituted the main series, the length of which was determined in each case by a number of uncontrollable factors such as the amount of negative adaptation of the reflex. In certain cases all four instructions were given a subject before the experiment ended.

After the experimenter had made sure that the instructions were understood and that the subject was properly adjusted in the apparatus, the experiment began. Approximately 2 seconds after the light was turned on (as determined by a stop-watch), the release of the photographic pendulum broke the circuit which produced the noise stimulus. The photographic record was then removed and labeled; the subject reported on his set for that reaction; and the apparatus was prepared for the next trial, which followed 30 seconds later. Each day's series, consisting of 60 trials, was divided into four periods with 2-minute rests between. A report from the subject on his general observations during the experiment concluded the day's session. Each daily series occurred at approximately the same hour for a given subject.

Apparatus

With the exception of the stimulating mechanism, the timer, and the Celotex booth inclosing the recording system the apparatus used in this experiment has already been described (15, Chap. I), and will be summarized briefly. (1) The lid-response lever is a light wooden stick, one end of which is attached horizontally and parallel to the upper eyelid with adhesive tape, while the other end turns on an axis and carries a concave mirror which reflects a beam of light to recording sensitive paper. (2) The photographic recording system consists of a moving plate suspended on double pendula and carrying photographic paper on which the lid response, time lines, and moment of occurrence of the stimulus are recorded. (3) The stimulus device for producing a noise of constant intensity is pictured in the diagram of Figure 2, and consists of three circuits. Circuit 1, containing a primary source, the magnets of a telegraph relay, an ammeter, and variable resistance, is broken when an offset from the recording pendulum strikes the switch, *S*. When this switch is closed, the armature of the relay, *R,* is drawn against the magnets and closes Circuit 2 through the primary of a Harvard inductorium. Before each trial the ammeter was read and the resistance was adjusted to give 0.6 amperes in Circuit 2. The current in the secondary

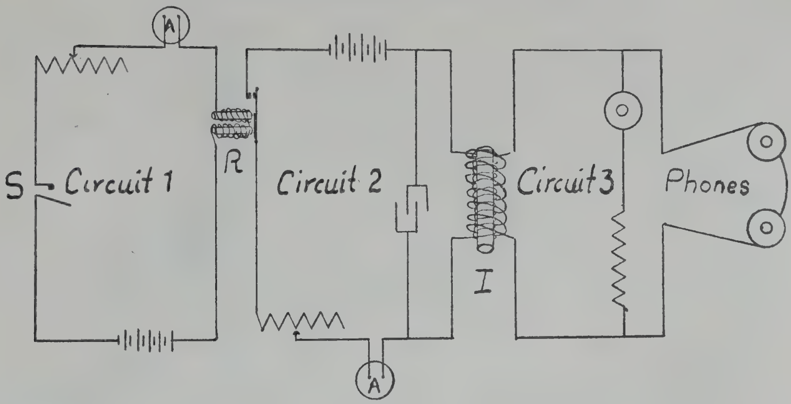

FIGURE 2
Diagram of Apparatus for Producing Noise Stimulus
See text

Circuit 3, induced when Circuit 2 is broken, activates the diaphragms of a pair of Western Electric headphones (2200 ohms resistance) which are placed in series with the secondary of the inductorium and in parallel with a Dodge mirror recorder and 30,000 ohms fixed resistance. The moment of occurrence of the noise in the headphones is recorded on the sensitive paper by means of this mirror recorder which consists of one receiver of a Western Electric headset with a pivoted mirror resting against its diaphragm and reflecting a beam of light which is deflected with movements of the diaphragm (15, p. 9). Noise in the headphones, which would result from closing Circuit 2, is avoided by short-circuiting the secondary circuit between each trial. (4) Timing device—vertical shadows of the spokes of a toothed wheel, rotating before a light source, mark 5-sigma intervals on the records. This toothed wheel is propelled by a motor, run synchronously with an electrically driven 100 d.v. tuning fork. (5) Housing—a booth of Celotex houses the recording pendulum, all relays and releasing mechanisms which might produce noise. It is solidly constructed and the experimenter may enter and close the door securely so that there are no openings. A piece of fine plate glass covers the slit in the wall of the booth through which the recording lights pass to the photographic paper.

Results

On examining the attributes of reflex and voluntary response, it soon became apparent that the most striking differences were to be

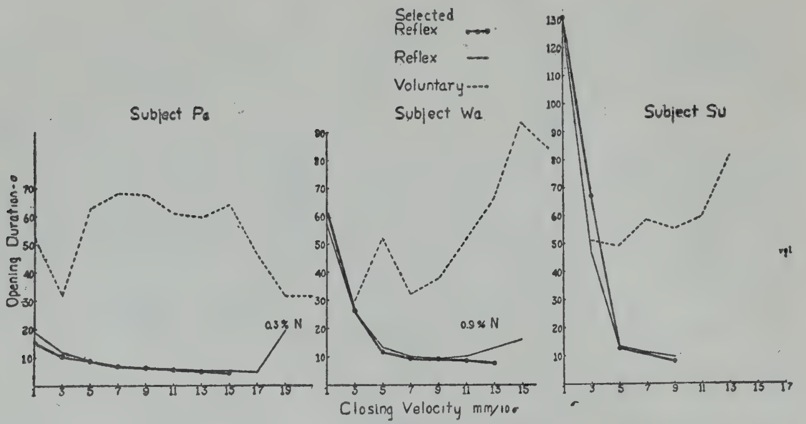

FIGURE 3

THE RELATION BETWEEN CLOSING VELOCITY AND OPENING DURATION

The relation between closing velocity (millimeters of closure in 10 sigma) and opening duration (time in sigma for lid opening to 2 millimeters) in reflex responses (all short latency responses, solid lines); in selected reflexes (all short latency responses without superimposed irregularities), solid lines through circles; and in voluntary responses, dotted lines. The values are averages of opening duration of responses falling within group intervals of 2-mm. closing velocity.

found in the correlations between certain of the individual attributes, and that the statement of these relationships gave a more accurate impression of the differences in response than did the separate treatment of each response characteristic. We shall, therefore, present first the data on the relations between attributes and later the consideration of differences in the distribution of the single attributes themselves.

I. *Relations between Attributes of Response*

1. *The relation of closing velocity to opening duration in reflex and voluntary responses.* Even casual inspection of the configuration of the lid reflex suggests the existence of a relationship between closing velocity and opening duration, and accurate measurement establishes the fact that as the velocity of closure increases the duration of the opening phase decreases, i.e., the more rapidly the lid closes, the more rapidly it opens to the point selected for measurement. The solid lines of Figure 3 represent this relation in the reflex,[6] when *all first short latency* responses are included in the re-

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[6] The graphs of only three subjects are given as typical, since the remaining subjects show no exceptions to these results.

flex group. It will be noticed, however, that in subjects Pa and Wa the curves change direction at the very end in the region of the highest closing velocity. An investigation of the small number of records responsible for this change (0.3% in one case and 0.9% in the other)[7] reveals the fact that they are all very irregular in form, a feature which has been shown previously to be characteristic of voluntary reactions (15). The suggestion is, then, that these few responses, which cause the only reversals in the curves, are probably voluntary reactions to the set and not reflexes at all. When such reactions are eliminated from the reflex group, the curves represented by the solid lines through circles result. Here the terminal rise entirely disappears and the relation between opening and closing is consistent, though not statable in mathematical terms, since the curves show different slopes for subjects reacting under different instructions. The present experiment is not designed to determine whether the instructions are responsible for this difference, but the fact that consistency exists for the limited number of subjects reacting under the same instructions suggests this as a plausible explanation.

The broken lines of Figure 3 represent the relation of the same attributes in voluntary response. Both the consistency and the direction which appear in the reflex curves are changed in the voluntary, for the response of rapid closure is not necessarily that of short opening duration. Rather, where there is any predominating tendency, as in subject Su, Figure 3, the responses of more rapid closure tend to be those of greater duration of opening, so that a relationship which has proven to be characteristic of the reflex has disappeared in voluntary lid reactions.

2. *The relation between amplitude and opening duration.* Amplitude or extent of closure in the reflex bears practically the same relation to opening duration as does closing velocity, i.e., as the amplitude of the reflex increases, its opening duration decreases. It is difficult to say what the relation may be for voluntary responses which often begin before the lid has returned to its base line, a fact which makes impossible the accurate determination of amplitude. Closing velocity may, however, be determined in many such cases, so that for the present purposes of comparing reflex and voluntary response it is more useful than amplitude.

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[7] Though few in number they greatly affect the course of the curve because they all fall in the region of high velocities where N is very small.

Though the point of major concern at this time is this difference of the relationship between attributes of reflex and voluntary response, a further important problem is involved in determining the mechanism which underlies the opening-closing relationship. Five possible explanations suggest themselves and these will be sketched very briefly, for their detailed discussion must await further experimental results. In the first place, it is possible that the opening of the lid after reflex closure is a voluntary response to the same noise stimulus which causes closure. Since opening commonly begins from 75 to 100 sigma after the stimulus, the latency is within possible voluntary limits, though below the average values. This explanation seems inadequate, however, chiefly because of the quantitative relation between opening and closing, which requires either that the two responses are dependent in the same degree on a common antecedent or that the one is the necessary antecedent of the other. In order that the stated relationship between a reflex closure and a voluntary opening to the same stimulus should exist, therefore, both responses would have to be affected in the same fashion by the stimulus and by all inhibitory and facilitative influences intervening between the stimulus and each response, a situation which is quite contrary to the known facts about differences in reflex and voluntary reactions in general, and hardly to be expected in view of the different levels of integration involved.

Secondly, it is possible that both are reflexes to the same stimulus,[8] the opening reaction being mediated by a longer path, with a consequently longer latency. In view of the relatively short distances involved, it is difficult to believe that the impulses over the pathway from the midbrain nuclei of the eighth nerve to the nuclei of the third nerve and thence to the levator muscle would require twice as much time as that from the seventh nerve nuclei to the orbicularis muscle. The possibility cannot be completely neglected, however, especially in view of the fact that reflex opening movements may be delayed simply by the prepotency of antagonistic movements of closure.

There is a third, related, but somewhat different, type of explanation of the facts in terms of a common antecedent condition. Sherrington (21, p. 206) and others have described the phenomenon of successive induction in spinal reflexes. In the absence of afferent connections in the reacting muscles it was found that excitation of a flexor reflex was followed by the exaltation of the antagonistic extension reflex, i.e., excitation of a center was followed by a tendency to its inhibition and the simultaneous excitation of the previously inhibited antagonist. Since the antagonistic muscles of the lid are apparently in a relationship of reciprocal innervation (18, 19, 20), such an effect as this is quite possibly operative, though, in the absence of any experimental evidence for it in the lid, such a statement is purely conjectural. The inhibition of the levator incident to excitation of the orbicularis may be followed by increased excitability of the levator and inhibition of the orbicularis. Presumably, the mechanism would operate in whatever common center is responsible for the reciprocal innervation of the two muscles.

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[8] This alternative was suggested in conversation with Dr. G. R. Wendt.

Isserlin (11) has discussed and criticized the suggestion made by various investigators, notably Rieger (16), that antagonistic movement following contraction of an agonist is a kind of elastic rebound of the antagonistic muscle which has been stretched. A purely mechanical factor such as this may be operative, but in the intact organism active processes are doubtless of greater importance.

It is the possible reflex effect of such a stretch which constitutes the fifth and final possibility to be discussed. In 1923 Liddell and Sherrington (13) reported that when a muscle with intact afferent paths was subjected to stretch it reacted reflexly with an increase in tension. In other words, the stretching set up stimuli intramuscularly which tended to produce contraction of the muscle. In the present case it is clear that, in closing the lid, the levator muscle is invariably elongated. This stretch might well stimulate the sensory endings sufficiently to cause contraction of the muscle and consequent opening of the lid. The demonstrated relation between the velocity and amplitude of closure and opening duration (or rate) in the reflex rather favors this explanation, since Liddell and Sherrington found that the phasic portion of the reflex was more pronounced in response to rapid stretch than when the stretch was delivered slowly.

The examination of the stretch reflex in the lid would require deafferentation of the levator muscle, which involves technical difficulties, and the investigation of successive induction is similarly complicated. By unilateral section of the seventh nerve it should be possible, however, to observe a great deal about the presence and latency of opening in the absence of immediately preceding closure, which should throw light on the dependence of one on the other.

Additional information will undoubtedly be provided by simultaneous records from the upper and lower lids, inasmuch as the lower lid is activated only by the orbicularis muscle, while the upper lid involves the levator as well. Without the results from such experiments further discussion is unwarranted.

Whatever the mechanism may prove to be in the reflex, it seems likely that its disruption in voluntary reaction is due to a continuation of impulses to lid closure sufficiently intense and persistent to inhibit the opening tendency which is doubtless operative here as in the reflex. Indeed, there are numerous records (see Figure 6, No. 2) showing slight opening movements at the top of the voluntary curve which are inhibited promptly and often repeatedly by antagonistic movements of closure.

3. *The relation between closing velocity and amplitude.* It might be expected from the above facts that the relation between

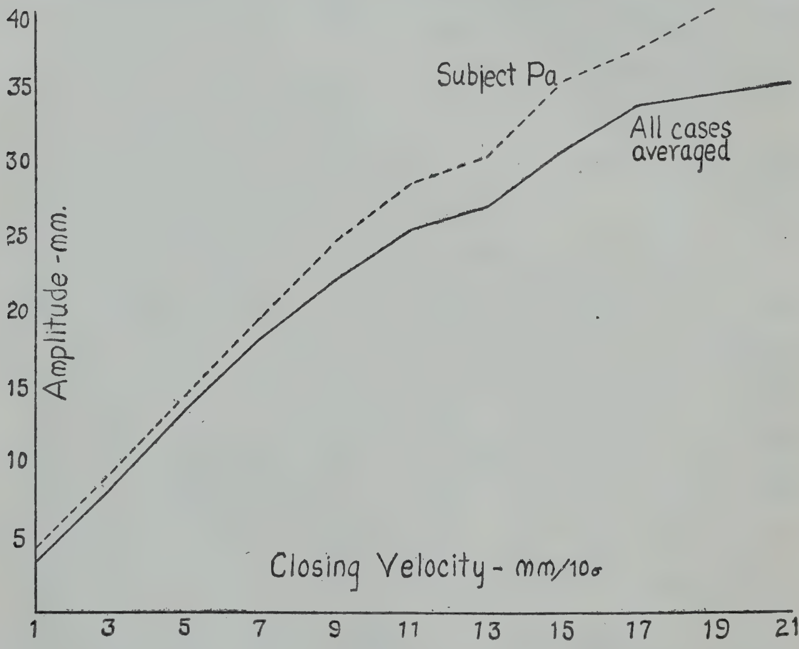


FIGURE 4

THE RELATION BETWEEN CLOSING VELOCITY AND AMPLITUDE IN REFLEX RESPONSE FOR A TYPICAL SUBJECT (DOTTED LINE), AND FOR THE AVERAGE OF SIX SUBJECTS (SOLID LINE) closing velocity and amplitude should be close in the reflex. The accompanying curve, Figure 4, shows this to be true, the relationship being practically linear within the smaller and medium-sized amplitudes and closing velocities. At the high values, however, the amplitude increments are not as great as the increase in closing velocity so that the curve departs from linearity in this region. The fact is doubtless due to the presence of a mechanical limitation to further closure of the lid when maximum closure has once been attained, a situation which does not affect the closing velocity of the response. This suggests that closing velocity might serve in such cases as a more reliable measure of amount of response than does amplitude alone. It is at least a more available measure in situations where the amount of decrement in the amplitude of the second response is seriously in doubt because its base line is not known and its extent is limited by the width of the eye slit.

Here again it is difficult to be certain of the nature of this relation in the voluntary reaction because of the difficulty mentioned above in determining amplitude in a secondary response. Other investigators, such as Isserlin (11) and Koch (12), have shown, however, that a similar relationship does exist in voluntary movements, though Koch points out that there is no *direct* proportionality between excursions and velocity of eye-movements.

II. *Comparison of Absolute Values of Response Attributes in Reflex and Voluntary Reaction*

1. *The average duration of opening in the reflex is shorter than that of the voluntary response in the following situations*:

a. In all subjects the uninstructed reflex responses recorded at the beginning of a series show clearly shorter opening duration than any of the early series of voluntary response. In other words, in the absence of repetition, the distinction between reflex and voluntary lid response, in terms of opening duration, is clear and consistent. This fact is shown in Table 1, in the three curves of Figure 5, which are representative of subjects reacting under different instructions, and in Figure 6.

b. For subjects reacting under instructions to wink voluntarily (*Ib,* neutral regarding duration) this relation holds true throughout the experiment, about 1080 responses. (See Subjects Go and Pa, Table 1, and Pa, Figures 5 and 6.) For this group the differences between reflex and voluntary average opening duration

TABLE 1

DAILY AVERAGES OF OPENING DURATION IN SIGMA UNDER INSTRUCTIONS I*a* (RELAXATION) AND I*b* (TO WINK VOLUNTARILY—DURATION UNINSTRUCTED)

For Reflex and Voluntary Responses

Subject	Series →		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Go	Reflex	Mean	8.2**	7.8	11.9	10.8	12.1	11.0	11.8	12.7	11.5	13.0	12.2	14.3	15.2	16.8	17.0	12.0	14.9	11.7	14.5
		S.D.	1.4	1.6	4.0	2.7	6.0	2.7	3.9	3.9	6.1	3.2	3.2	3.8	6.0	8.5	7.7	3.9	5.4	5.1	5.0
		N	41	60	56	55	52	54	55	55	58	44	19	15	14	11	4	7	9	24	15
	Voluntary	Mean	65.7**	38.9	42.5	36.0	27.9	37.6	30.1	34.3	30.7	30.3	35.8	30.6	42.1	60.8	85.6	48.3	31.5	35.6	40.5
		S.D.	23.3	16.6	20.7	20.4	18.9	18.4	17.1	15.8	12.1	20.4	20.4	15.2	18.5	26.4	35.2	21.5	11.3	19.8	26.4
		N	38	59	56	52	56	59	57	55	57	55	27	35	35	33	13	24	21	30	24
Pa	Reflex	Mean	7.4*	6.9**	6.5	8.3	10.6	9.3	11.5	11.3	11.2	11.9	11.7	15.9	11.9	11.5	10.0	12.7	8.2	10.3	
		S.D.	4.0	2.2	1.7	2.6	3.6	2.6	4.9	3.8	3.5	3.1	3.8	7.3	2.2	4.8	1.6	1.5	2.3	2.8	
		N	58	59	57	57	49	54	43	39	26	21	12	14	9	6	3	4	4	10	
	Voluntary	Mean		37.1**	76.8	72.8	71.4	78.6	78.1	47.8	48.2	67.5	69.6	76.4	61.8	39.9	37.3	35.7	42.2	38.7	
		S.D.	none	21.3	69.6	41.9	43.3	39.4	51.9	30.0	35.9	38.0	28.8	56.0	40.0	29.8	33.5	25.9	25.5	28.9	
		N		51	56	55	44	54	41	45	49	41	30	44	26	34	9	8	18	30	
Sm	Reflex	Mean	16.5*	14.9*	15.7	14.4	12.7	9.8	11.8	27.3**	17.8	6.4	11.9								
		S.D.	7.0	6.9	9.0	4.9	4.8	3.1	3.7	35.7	12.2	3.6	14.9								
		N	59	58	57	54	53	50	33	20	17	10	8								
	Second responses	Mean		98.7*	84.8	50.0	81.4	56.6	113.4	121.1**	109.0	131.3	166.5	170.5	272.7						
		S.D.	none	19.7	32.11	21.7	38.1	30.5	84.0	64.3	62.7	27.3	34.4	64.6	116.8	*See Table 3*					
		N		4	13	12	30	32	37	25	14	15	10	33	54						
Su	Reflex	Mean	20.4*	119.1	63.1	99.2	100.9	88.8**	96.7	114.0	151.7	113.6									
		S.D.	10.3	63.0	43.5	57.6	67.1	57.6	56.9	67.0	50.2	48.7		Reflex responses too small for measurement							
		N	14	42	39	50	29	30	28	20	14	9									
	Voluntary	Mean						61.8**	65.3	72.9	53.9	47.4	45.9								
		S.D.	none					22.4	24.7	35.3	18.6	26.4	13.7								
		N						11	19	7	18	27	35								
Wa	Reflex	Mean	17.2*	13.8**																	
		S.D.	7.9	6.5	See Table 3 for reactions under other instructions																
		N	26	27																	
	Voluntary	Mean		202.9**																	
		S.D.	none	62.0																	
		N		24																	
He	Reflex	Mean	19.1*																		
		S.D.	6.8		See Table 2 for reactions under other instructions																
		N	30																		
	Voluntary	Mean																			
		S.D.	none		Sustained closure (see Figure 6, record 6)																
		N																			

*Instructions to relax (I*a*) given here and in successive series.

**Instructions to wink voluntarily (I*b*) given here and in successive series.

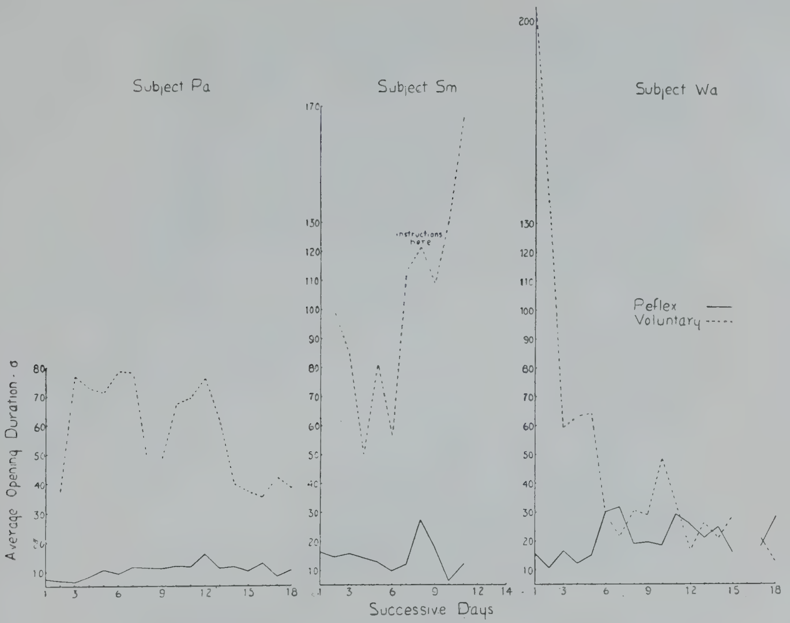
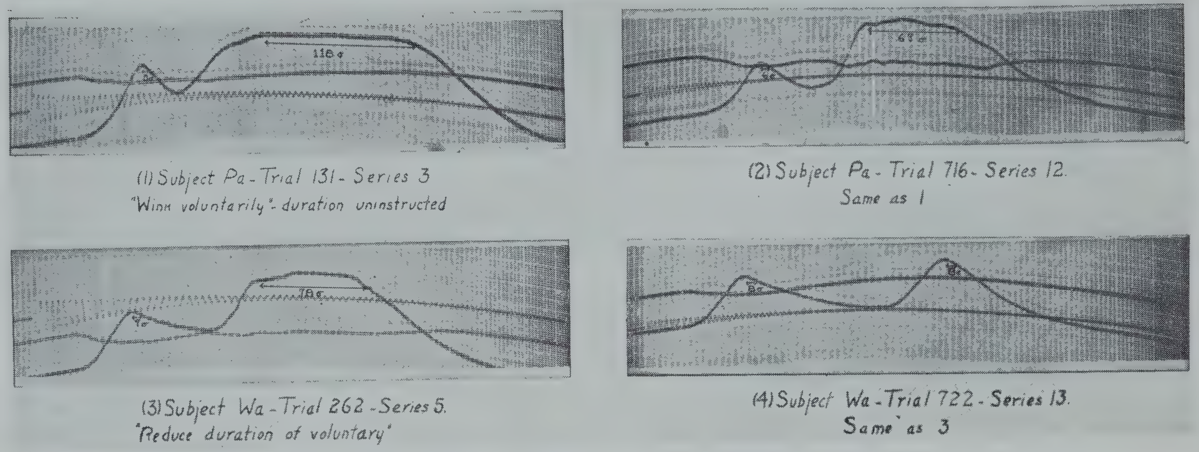


FIGURE 5

Opening Duration in Reflex and Voluntary Response on Successive Days of Repetition

Subjects Pa and Sm, uninstructed durations. Subject Wa instructed to open lid promptly after voluntary closure

on each experimental day are statistically reliable throughout. The same is true of Sm, reacting under relaxation instructions (Table 1 and Figures 5 and 6, No. 7). Though not instructed to react voluntarily, this subject began to give secondary responses during the second day's series and did the same every day thereafter. On questioning, he reported that he was not trying to wink but that he was tense and expectant in spite of attempts to relax. These secondary responses show consistently longer opening duration than the reflexes and are indistinguishable from the responses subsequently given by the same subject under instructions to wink voluntarily. The facts suggest the presence of some antecedent common to this situation and to that in which the subjects were instructed to wink voluntarily, which is probably the "attitude" of expectancy.



(1) Subject Pa - Trial 131 - Series 3
"Wink voluntarily" - duration uninstructed

(2) Subject Pa - Trial 716 - Series 12.
Same as 1

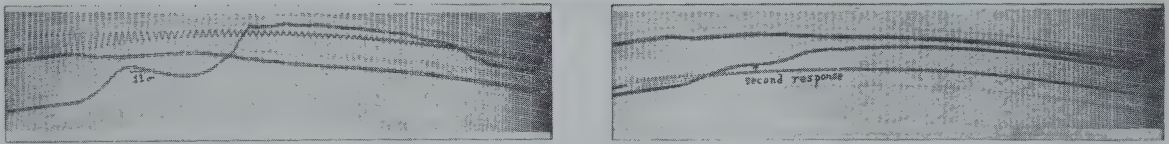
(3) Subject Wa - Trial 262 - Series 5.
"Reduce duration of voluntary"

(4) Subject Wa - Trial 722 - Series 13.
Same as 3

FIGURE 6

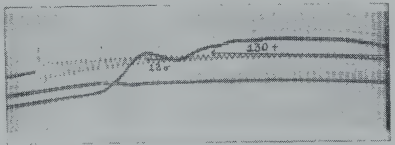
RECORDS OF SUBJECTS REACTING UNDER VARIOUS INSTRUCTIONS

Stimulus indicated by beginning of vibration. Upper dark line, record from lower lid; lower dark line, from upper lid. White ordinates, time lines at 5-sigma intervals. Opening duration values indicated.



(5) Subject He-Trial 97- Series 3.
"Increase duration of reflex"

(6) Subject He-Trial 555- Series II.
Same as 5



(7) Subject Sm-Trial 177- Series 3.
Uninstructed responses

FIGURE 6 (*continued*)

TABLE 2

OPENING DURATION IN SIGMA—INSTRUCTIONS II (TO KEEP LID CLOSED AFTER REFLEX CLOSURE)

For Reflex Responses

Subject	Series →		I*a*	I*b*	2	3	4	5	6	7	8	9	10	11	12	13	14
He	Reflex	Mean	19.3*	17.6**	24.0	17.8	18.9	35.8	29.4	23.5	30.5	45.8	34.3	37.0	26.3		66.9
		S.D.	6.9	6.3	11.7	8.3	5.7	10.6	9.9	5.5	14.5	21.5	9.1	2.0	2.7		28.4
		N	29	29	33	28	35	12	9	6	2	27	3	2	3		11
		Voluntary opening durations always longer than reflex.															

*Relax (I*a*); **Keep lid closed (II).

TABLE 3

OPENING DURATION IN SIGMA—INSTRUCTIONS III (TO OPEN LID PROMPTLY AFTER VOLUNTARY CLOSURE)

For Reflex and Voluntary Responses

Subject	Series →		I*a*	I*b*	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
Wa	Reflex	Mean	17.2*	13.8**	10.7***	16.2	12.1	15.0	30.0	31.6	18.9	19.5	18.4	29.1	25.9	21.0	24.8	16.0	records ruined	17.8	27.9
		S.D.	7.9	6.5	5.9	12.5	8.4	9.4	19.6	34.9	16.7	17.6	10.8	13.0	16.6	14.5	12.5	8.7		9.5	16.4
		N	26	27	57	57	55	44	57	52	52	48	42	18	43	47	30	48		40	15
	Voluntary	Mean	none	202.9**	136.5***	59.2	63.2	64.1	29.0	21.5	30.3	28.9	48.6	32.9	16.6	26.3	21.0	28 5		21.1	12.8
		S.D.		62.0	47.4	23.8	22.7	23.0	15.0	8.9	18.0	15.7	12.9	14.4	7.7	13.1	11.3	12.8		8.9	3.2
		N		24	58	58	58	45	52	43	27	58	56	54	54	60	57	49		58	55
Sm	Voluntary		See Table 1 for uninstructed responses													*M*	90.1***	126.7	84.9		
														Instr. III	*S.D.*	47.0	39.5	48.5		
															N	54	37	49		

*Relax; **Wink voluntarily; ***Open lid promptly.

c. Reactions obtained under instructions to keep the eyes closed after reflex closure (Instruction II) show the same relation between reflex and voluntary opening duration. The instructions produce secondary responses which resemble in latency and opening duration those obtained under instructions to wink voluntarily. (See No. 5, Figure 6, and Table 2.) With practice this response becomes shorter in latency until it overlaps the first, though the second response usually remains distinct, as in Figure 6, No. 6. In such a case, however, the duration of the reflex is unavailable for measurement, though the response is clearly indentifiable as a mixed type and will not be confused with an uncomplicated reflex response because of the configuration as well as the genetic history of the reaction. The opening durations of the secondary responses have not been measured because of their tendency to continue beyond the limits of the record under these instructions. They are, however, the longest opening durations obtained in the experiment, and are always longer than the reflex.

2. *The average opening duration of reflex and voluntary responses tends to become indistinguishable under the following conditions:*

a. We have indicated the relationship existing between closing velocity and opening duration and between amplitude and opening duration in the reflex. In consequence of these facts, when closing velocity and amplitude decrease markedly in the course of repetition, that is, when negative adaptation takes place, the opening duration of the reflex increases to such an extent that it exceeds that of the voluntary response. Any eventual generalization about absolute values of opening duration in the reflex and the voluntary responses must, therefore, involve a statement as to the minimum amplitudes and closing velocities within which the generalization holds true.

b. When subjects are instructed to open the lid promptly after voluntary closure (Instruction III) this is not immediately possible. (See Figure 6, No. 3, and Table 3). With practice, however, Wa, the only subject who continued under these instructions through 18 series, was able to reduce the duration of his voluntary response until it was equal to, or less than, that of the reflex. Sm, reacting for 180 trials, and other subjects in short series under these instructions reduced the opening duration somewhat but did not succeed in reaching that of the reflex within the limits of their practice. (See Table 3.)

It is interesting in this connection to find that Garten made observations not unlike these in his early study on lid reactions, the differences apparently being due to the fact that none of his subjects were given sufficient practice to succeed in reducing the durations of their voluntary responses. He says (7, p. 486):

> "In vielen Fällen bessassen die willkürlichen Lidschläge der gleichen Person, auch wenn diese beabsichtige möglichst kurze, regelmässige Lidschläge auszufuhren, abweichende Form. *Namentlich war die Zeit des Herabgehens des Lides oft eine auffallend lange.*"

Weiss (24), without defining the exact meaning of the measure, reports for voluntary lid closure an average *Geschlossensein* of 0.031 sec., with a maximum of 0.055 sec. and a minimum of 0.014 sec. There is no available basis of comparison with reflexes.

Though not concerned with a comparison between reflex and voluntary behavior, still other investigators have made observations on the course of movement which must be considered briefly in relation to the results which we have obtained. Isserlin (11), Rieger (16), and others have noticed, for instance, that voluntary extension of a limb tends to be followed by flexion or flexion by extension,[9] i.e., that contraction of a muscle is commonly followed by the contraction of its antagonist (the *Rückstoss*). Isserlin shows further that the *Rückstoss* occurs only when the first contraction attains a certain velocity. The phenomenon, therefore, appears to be similar to the brief opening duration which we have suggested is most often characteristic of the *reflex* in the lid. Certain facts indicate that the apparent contradiction here is not real. In the first place, in the absence of data on the reflex obtained under the recording conditions of Isserlin's experiment, there is no basis for determining how the *Rückstoss* in his voluntary reactions would compare with the same phenomenon in a reflex finger movement, and there are no quantitative measures given by which a comparison of absolute values of this feature of his voluntary response with ours can be made. It can only be said, therefore, that certain of his published curves resemble our typical voluntary responses very closely in the prolongation of the initial contraction, while others indicate a more immediate onset of antagonistic contraction like our responses obtained under instructions to open the lid promptly.

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[9] Isserlin (11, pp. 31 ff.) discusses some of the other studies. The same phenomenon has been observed in various sorts of movement. Dodge (4, p. 374), for instance, observed in eye-movements, "die kleinen Haken am Ende der langen Schwenkungen, wie auch am Ende vieler der kürzen."

The same may be said of Wachholder's (23) more recent results on voluntary limb movements. Out of the twenty-one records published here, only four show a marked difference from our voluntary curves in the prolongation of contraction, and all of the four are clearly distinguishable from our reflex responses by the presence of what Wachholder describes as "regelmässigen pendelartigen Hin- und-Herbewegungen." There is no single curve showing the relatively smooth downward course which usually characterizes the lid reflex. Both Isserlin and Wachholder have used a wide variety of instructions, and both series of experiments suggest that the prolongation of contraction without interruption by antagonistic movement is most marked where the instructions are of the sensory type where speed is not emphasized. In other words, these researches indicate that the speed instructions, such as we have used, are the most unfavorable conditions under which to obtain uninterrupted contraction of the agonist. The case of our subject Sm is also suggestive in this regard. Though he was making no effort to wink voluntarily, his secondary responses (presumably in response to his reported attitude of expectancy) showed the longest average opening duration of any subject except He, who was instructed to prolong closure. It appears, therefore, that the differences which we have obtained between the opening duration of reflex and voluntary response have appeared *in spite* of the speed instructions which have been shown elsewhere to be unfavorable for the appearance of the difference.

Of further importance is the fact that the studies referred to deal with different reaction systems from ours, for even though the distinction which we have made between reflex and voluntary responses may be valid for the reactions of the lid, it is not necessarily true of other kinds of movement. If, for example, the tendency to the *Rückstoss* originates in the contraction of the agonist, as the results suggest, this tendency might well be more pronounced in the large muscles of the limbs (probably richer in proprioceptors) than in the small lid muscles. In this case the same voluntary impulses to continue the initial contraction would overcome the relatively weak antagonistic tendency in the lid more readily and more frequently than the stronger tendency in a large muscle, with the resultant greater prolongation of agonistic contraction in the voluntary re sponse of the lid. Care must be taken, therefore, to limit generalizations about this point to the conditions of the present experiment.

3. *Average closing velocity of voluntary responses is greater than that of reflexes.* We have spoken of the relation of opening to closing velocity in voluntary response. It is of further interest to compare the absolute values of closing velocity for the two types of response under the conditions of the present experiment. A plot of the distribution of these values, which cannot be reproduced here for lack of space, indicates that, in spite of obvious overlapping, the modes of the reflex and voluntary responses lie at distinctly different points, that of voluntary responses falling between 0.7 and 0.9 mm. in 10 sigma, compared to the reflex mode at 0.3 mm. in 10 sigma. The fact is even more apparent in the daily averages of closing velocity for each subject. With few exceptions, the average closing velocity of the reflex is less than that of the voluntary response.

Garten (7, p. 487) reports that the lid reflex elicited by blowing on the cornea has "the same steep rise" as the voluntary movement. In contrast to these results and to our own, Schlosberg (17, p. 487) and Wendt (25, p. 41) have both observed that voluntary and conditioned knee-jerks have slower rising curves of contraction than the unconditioned response. Certain possible factors in these different results suggest themselves: (1) We have mentioned the importance of a difference in reaction systems. (2) Of greater importance, however, is the possibility that the degree of "effort" involved in a voluntary response may be a determining factor in velocity of contraction. In our experiment the subjects acting under speed instruction were relatively alert. Isserlin (11) has found that reaction curves of the finger occurring under "sensory" *Einstellungen* are flatter and the velocity of the contraction slower than in the motor type of reaction which produces a more rapidly rising curve and evidence of greater energy. Angell (1) has obtained similar results on the effect of *Einstellungen.* This would indicate that the velocity of contraction was largely determined by the set. Against this stands the fact, however, that in the present experiment there was no tendency for the closing velocity of the voluntary response to decrease as the experiment progressed and as the subjects became more relaxed. On the contrary, both closing velocities and amplitudes in the voluntary responses increased slightly so that differences in reflex and voluntary responses were greater at the end of the experiment than in the beginning. (3) It is certain, however, as many have pointed out, that wide variability is characteristic of voluntary reactions. Thus it may be that no one of the experiments discussed has investigated the complete range of voluntary response velocities and that all the results may be correct within different portions of the limits of variability.

4. *Average amplitudes of the voluntary response are greater than those of the reflex.* The distribution of amplitude for reflex and for voluntary response is similar to that of closing velocity, with the amplitude of the voluntary responses greatly exceeding that of the reflex.[10] This is true both of the total distribution of responses and of the averages of successive days of practice. The same qualifications must be made here, however, as for closing velocity, for further exploration of voluntary "sets" would undoubtedly extend the limits of variability of voluntary amplitude.

CONCLUSIONS

The present experiment has defined the lid reflex to sound tentatively as the short latency response dependent on a noise stimulus above a minimum intensity, and the voluntary lid response as a longer latency reaction dependent primarily on instructions to wink voluntarily, and has examined the objective characteristics of these responses recorded photographically. The following facts have been demonstrated:

1. In the lid reflex to sound certain characteristics of the response bear a consistent relation to each other, a relation which is different in voluntary responses obtained under the same conditions:

a. As the closing velocity of the reflex increases, its opening duration decreases. The relation is reversed or absent for the voluntary response.

b. As the amplitude of the reflex increases, the opening duration decreases.

c. Closing velocity of the reflex increases directly as amplitude increases up to the region of large amplitudes and fast velocities where the proportion of increment in amplitude is smaller than in closing velocity. This suggests that amplitude is subject to mechanical limitations not affecting closing velocity.

d. Consideration of the possible mechanisms underlying these relationships in the reflex suggests five alternatives in need of experimental attack: (*a*) opening may be a voluntary response to the same antecedent conditions causing closure; (*b*) opening may be a reflex response to the same antecedent conditions causing closure; (*c*) it may be due to simultaneous induction following reciprocal inhibition of its nerve center during closure; (*d*) it may be caused by mechanical elastic rebound of the levator muscle which is stretched by closure of the lid; finally, (*e*) opening may involve a "stretch reflex" whose stimulus is the elongation of the levator muscle incident to closure. Experiments are suggested to throw light on the facts.

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[10] Hilgard has observed that the conditioned response has a greater average amplitude than the reflex (9, p. 26).

2. Under certain conditions of the present experiment, the reflex shows:

a. smaller average amplitude,

b. smaller average closing velocity,

c. shorter average opening duration than the voluntary response.

3. Under the following conditions the opening duration of the reflex may *not* be shorter than that of the voluntary responses:

a. After a subject has practiced under instructions to open the eyes quickly after closure, the opening duration of the voluntary decreases below that of the reflex.

b. When the reflex responses have undergone negative adaptation the resultant decrease in closing velocity and increase in opening duration (see 1*a* above) make the opening duration of the reflex exceed that of the voluntary response.

4. The opening duration of the reflex response is shown to be less readily modifiable by the instructions used here than is the opening duration of the voluntary response.

5. From these facts it is clear that there are conditions under which the attributes of reflex and voluntary responses are distinguishable, but that certain conditions may be arranged under which the response attributes are practically the same. The bearing of these facts on a general evaluation of the categories reflex and voluntary will be discussed in a theoretical paper (14).

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LES RÉACTIONS RÉFLEXES ET VOLONTAIRES DE LA PAUPIÈRE

(Résumé)

Comme résultat de l'emploi d'une technique photographique pour enregistrer le progrès de la réponse de la paupière au son laquelle a eu lieu après divers rensiegnement donnés aus sujets, le rapport présente des mesures seigneuses de la latence, de l'amplitude, de la vitesse de la fermeture, et de la durée de l'ouverture des réactions réflexes et volontaires de la paupière. Les résultats indiquent que dans certaines conditions spécifiques on peut distinguer clairement ces deux groupes de réponse au moyen de leurs caractéristiques descriptives mais que des conditions peuvent être établies dans lesquelles on ne peut pas les distinguer en termes d'une seule caractéristique, quoique même dans ces circonstances la détermination de la relation d'un attribut à un autre (c'est-à-dire, la vitesse de la fermeture et la durée de l'ouverture) puisse identifier la réponse comme réflexe ou volontaire.

Peak

REFLEXIVE UND WILLKÜRLICHE REAKTIONEN DES AUGENLIDES

(Referat)

Die Verfasserin gebrauchte ein photographisches Verfahren zur Registrierung des Verlaufs der Reaktion des Augenlids auf Geräusche (sound) unter Erteilung verschiedener Anweisungen (various instructions) an die Versuchspersonen. Der Bericht überreicht sorgvolle Messungen der Latenzzeit, des Umfangs (amplitude), der endgültigen Schnelligkeit, und der anfänglichen Dauer reflexiver und willkürlicher Reaktionen des Augenlides. Die Befunde weisen darauf hin, dass, unter gewissen spezifischen Bedingungen, diese zwei Reaktionsgruppen sich an ihren beschreibbaren (descriptive) Eigenschaften klar unterscheiden lassen, dass aber Bedingungen errichtet werden können, unter denen sie in Bezug auf irgend eine einzelne Eigenschaft ununterscheidbar werden, obwohl die Bestimmung der Beziehung einer Eigenschaft zu einer anderen (z. B. der endgültigen Schnelligkeit zur anfänglichen Dauer) die Reaktion als eine reflexive oder eine willkürliche auch unter diesen Umständen zu identifizieren vermag.

Peak

TWO STUDIES IN RETROACTION:[1]

I. Influence of Partial Identity

II. Susceptibility to Retroaction at Various Grade Levels*

From the Psychological Laboratories of Yale University

Thelma A. Dreis

I. Influence of Partial Identity

In recent investigations in retroaction, attention has been directed toward demonstrating the theoretical inversion curve suggested by Robinson and by Skaggs.

The existence of retroactive inhibition was established in the work of G. E. Müller and A. Pilzecker in 1900 (6). Their findings led them to the conclusion that the results obtained are not due to the similarity between the syllables in the interpolated series and those in the original learning series.

DeCamp (2) found almost no retroactive effect with his material and concluded that too much stress has hitherto been laid upon the effect of retroactive inhibition. Nevertheless, he discussed the phenomenon and suggested an explanation of the process in the following words, "The effect of retroactive inhibition would vary directly as the relative identity of the neurological groups concerned" and " . . . we should expect retroactive inhibition to appear more readily where material similar to that learned is used for the interpolated work" (pp. 62-63).

The first attempt with enough conditions to determine experimentally the comparative influence of similar and dissimilar interpolation was made by Robinson (7) in which he concluded that the degree of retroactive inhibition is a function of the similarity between the original learning and the interpolated work and that further study would have to be made to decide at just what point in the variation

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[1] Acknowledgment. I wish to acknowledge my indebtedness to W. T. Heron of the University of Minnesota under whose direction these studies were made, and to Edward S. Robinson of Yale University who offered valuable suggestions in the revision.

*Recommended for publication by Edward S. Robinson, accepted by Carl Murchison of the Editorial Board, and received in the Editorial Office, March 23, 1932.

of similarity interpolation turns from a disintegrating into an integrating agency. Skaggs (9) tried this variation and chose activities for interpolation which, according to the ranks he gave them, showed greater negative retroaction with greater similarity between original and interpolated learning. Though he did not find the point of inflection between similarity and dissimilarity suggested by Robinson, he discussed and endorsed the theory.

The final and most recent study up to the time we began this investigation was that published by Robinson (8) in which he attempted a variation of conditions of increasing similarity; but he failed to find the expected inversion. He concluded with confidence in the theoretical function, though "there are demonstrable conditions under which such inversion is absent" (p. 312).

To date, no one has demonstrated the complete relationship from identity to complete dissimilarity of original and interpolated learning with the expected inversion, although a concave (not a convex) curve is indicated in combining the results of Robinson and Harden (3).[2]

In the present study, which was completed in March, 1930, we attempted to measure the influence upon recall of similarity between original learning and interpolation, and to compare recall after similar interpolation with recall after rest.

Method, Procedure, and Subjects. Various methods and instruments have been utilized to measure the amount of recall, recognition, or relearning after interpolation of similar and dissimilar material. In the following experiment, relearning as measured by the percentage of saving was the method employed.

For the particular problem under consideration, it was desirable to have an instrument of measurement which: (*a*) could be used for group testing; (*b*) would take a limited period of time; (*c*) would require little explanation and instruction; and (*d*) could be scored rapidly. A substitution or code test was devised to meet those needs. The Binet-Simon substitution of geometric symbols was used, but to prevent any chance of a practice effect where subjects might have learned the code the symbols were used in reverse order; so that what had been *A* in the original test was used as *Z* in this test.

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[2] For a review of the experimental literature see the bibliography of this article (3), also McGeoch (4) and his most recent study (5).

TABLE 1

CONDITIONS AND INTERPOLATION USED IN EACH CONDITION

Condition	Original learning	Interpolated test	Relearning
X	BMTXEWN	Rest	BMTXEWN
0	BMTXEWN	OVCFIKA	BMTXEWN
2	BMTXEWN	HG**T**LP**W**J	BMTXEWN
3	BMTXEWN	SR**B**U**ME**Z	BMTXEWN
4	BMTXEWN	P**M**Y**TB**Q**E**	BMTXEWN

The boldfaced interpolated letters are elements in common with the original learning.

To distribute the letters of the alphabet for five conditions of experimentation, division was made as shown in Table 1.

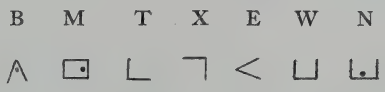
The tests were arranged on paper, size 8½ x 11 in., with the code at the top as shown in Table 2. Care was taken to distribute the test letters with due consideration for repetitions and juxtaposition of combinations.

Two columns like the above, of 80 symbols each, grouped by fours, constituted one test. The five conditions of experimentation, each of which was used with a different group of subjects, were arranged as follows (for convenience and clearness of reference, we shall speak of these conditions according to the number of elements common to both original learning and interpolation—0, 2, 3, 4, with *X* to designate the condition of rest):

Condition X involved the original learning test with a time limit of three minutes for work, three minutes for rest, and three

TABLE 2

A PART OF ONE OF THE SUBSTITUTION TESTS SHOWING THE CODE AT THE TOP AND A FEW LINES OF THE TEST



NTWE					BWTE				
WNBE					MNBW				
MTBX					TMEB				
NWXM					WNTX				
MBWX					TBWM				

minutes for work again. The original learning test was used twice in this arrangement. The tests had been previously clamped together so that the original learning test was on top, a blank sheet of paper was second in order, and a second copy of the learning test completed the series.

Condition 0 was used with a second group to test the results of interpolation of material with no elements in common with the original learning. The interpolated work followed immediately after the original learning, so no blank sheets were needed between the tests, and the series consisted of the original learning test, the interpolated test with no elements in common with the original learning, and the second copy of the learning test.

Condition 2 was used with a third group of subjects to test the effect of the interpolation of material with *2* elements in common with the original learning material. The test with 2 elements identical with the original learning was inserted between the end tests as in the above. With the same arrangement of tests

Condition 3 was used with a fourth group of subjects to test the effect of *3* common elements in original learning and interpolated work; and

Condition 4 was used with a fifth group to test the influence of *4* elements common to both original learning and interpolation.

Preliminary Investigation. A preliminary investigation was made to test the reliability of the measuring instrument and to ascertain the amount of retroaction that might be expected in the most promising of the above conditions. The tests of Conditions X and 0 were used in this introductory investigation.

The results of the preliminary experiment showed a fairly high reliability (.81±.047) for the testing instrument as shown by a Spearman rank-order correlation between learning and relearning in the first test, and an appreciable excess of saving (12%) in the condition of rest as compared with the condition which had the interpolation. From the results of this preliminary study, it was concluded that the tests employed could be depended upon for use in the experimental series to compare retroactive effects.

Experimental Series. The beginning classes in laboratory psychology were used for the experimental series. The five classes afforded the groups for the five conditions of the study.

The time for the experiment was selected so that a 20-minute period at the beginning of each class was utilized. All the classes were tested on two days, Monday and Tuesday, of one week so there would be little occasion for discussion of and speculation about the tests. No attempt was made to choose "stimulating hours" (10). The groups were taken at their scheduled hours, three groups in the morning, and two in the afternoon.

Before the classes entered the laboratory (all were tested in the same room) the experimenter wrote a sample of the test on the movable blackboard. (See Table 2.) This was then turned from the view of the class. Care was taken so that no symbol common to the test to be given was included in the symbols used for the practice demonstration.

After the class had assembled, two laboratory assistants passed out the test, face down, while the experimenter gave the following instructions:

> *A*. "Keep the papers upside down until I instruct you to turn them over. Print your name in the upper right hand corner on the paper as it lies before you. This is a learning experiment. You are to learn certain symbols which stand for the letters of the alphabet. At the top of the sheets (when you turn them over) you will find seven letters of the alphabet with the corresponding symbols similar to these [at this point the experimenter turns the blackboard so the sample demonstration can be observed by the class (see Table 2)].
>
> "Below you will find a series of groups of four letters like these. Beside each group, in the squares, you are to write the four symbols which stand for the letters of that group. Notice that there are two columns, so keep right on working.
>
> "Work as rapidly as you can when the signal is given. Try to learn the symbols which stand for each letter as you work. Are there any questions?
>
> "Now, when I give the signal 'Ready,' turn the paper over and work as rapidly as possible until I say 'Stop.'
>
> "Ready, start."
>
> *B*. The experimenter held a stop watch, and at the end of three minutes she said, "Stop. Now, when I give the signal, turn to the next test. Continue to substitute symbols as you did before. Work as rapidly as you can until I say 'Stop.' Ready. Go."
>
> After three minutes more the subjects were stopped, given the last paragraph of instructions (*B*) again, and allowed to work for three minutes on the last test. At the end of that time they were instructed, "Stop; turn your paper over; pass it to the left."

During the instruction between tests, a constant period of 30 seconds was maintained.

In the first condition which involved the rest period, the instructions had to be slightly modified. The preliminary directions (*A*) were the same, but after the first test of three minutes, the experimenter said:

> "Stop and turn to the blank sheet. This is a rest period. Now, during the next few minutes do not worry about your score. RELAX, assume a lazy attitude, and get away from the learning material. This material will be blocked out of the mind with difficulty at times, but try to meet experimental conditions as well as possible by assuming a discouraging attitude toward any intrusion by the learning symbols. You may converse if you care to. Do not talk about the experiment. Do not turn a page until you are directed to do so."

The three minutes of rest included the time taken by the experimenter to give the instructions. After two and one-half minutes the experimenter asked for the attention of the group, and gave instructions (*B*) for the last test. Papers were collected as in the other conditions.

Subjects. For the experimental series, 179 subjects were used, distributed as they were enrolled in their respective classes in elementary psychology laboratory. The division into the five conditions was as follows: 27 in Condition X, 44 in Condition 0, 24 in Condition 2, 43 in Condition 3, and 41 in Condition 4. All but 16 of the subjects were undergraduates; the majority of them were sophomores.

Results and Discussion. Our results are shown most clearly in the graphic representation of Figure 1. (See also Table 3.)

Two aspects of these data are apparent. Consider first Conditions

TABLE 3

PERCENTAGE OF SAVING IN EACH CONDITION AND ELEMENTS COMMON TO ORIGINAL LEARNING AND INTERPOLATION IN EACH CONDITION

Condition	Elements common to original learning and interpolation in each condition	Percentage of saving
X	— (Rest)	27.8
0	0	24.9
2	2	26.5
3	3	34.9
4	4	33.0

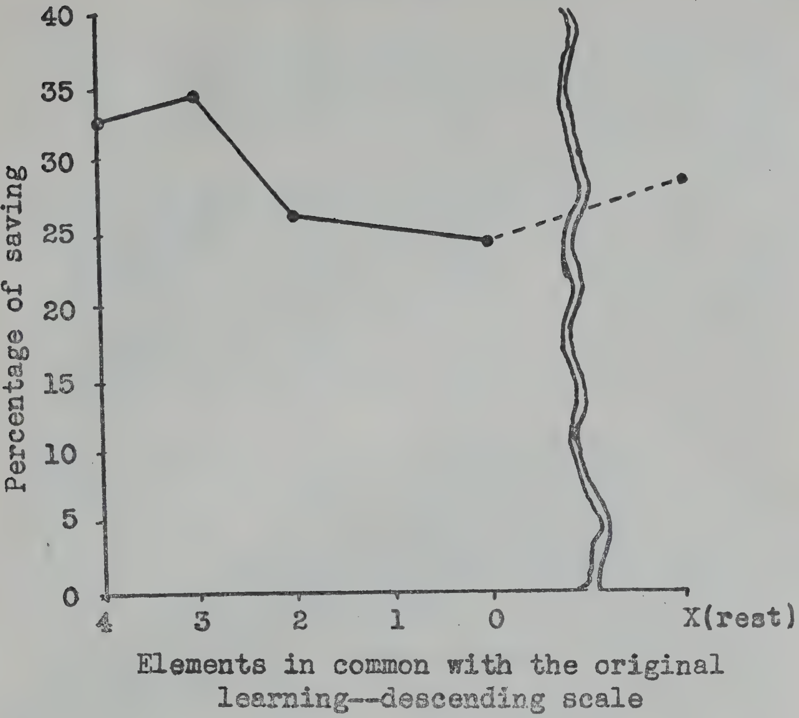


FIGURE 1

Showing the Relative Amount of Saving Found in the Five Conditions
(Precise values given in Table 3.)

0 to 4. As we increase the elements in common from 0 to 4, the general tendency is for saving to increase,[3] i.e., 24.9%, 26.5%, 34.9%, and 33.0%, as we approach the greatest similarity. (It is observed that an inversion of the relationship occurs in Condition 3 with a saving of 34.9% but the magnitude of the irregularity suggests that it is merely a statistical accident.) Therefore, in this series we find no inversion with increasing similarity of interpolated and original learning.

If, now, we consider the case where we have the same general type of interpolation, but with no letters in common with the original learning, as was true in Condition 0, and if we compare that condition with Condition X, we seem to have an increase in negative retroaction with decreasing saving (27.8% in Condition X and 24.9% in Condition 0).

---

[3] Percentage of saving is computed by dividing the absolute amount saved (relearning score minus original learning score) by the figure measuring the original learning.

This fall in the saving from Condition X to Condition 0, with the gradual rise in the curve from Conditions 0 to 4 (as similarity still further increases) would suggest that our complete function, including Condition X, might be interpreted as substantiating the Robinson-Skaggs Law. The difficulty of taking this as a verification of the law is that the base line represents a questionable synthesis of two distinct variables; that is, the variation in similarity from Condition X to 0 is certainly not within the same dimension as from 0 to 4.

The above analysis assumes that the five groups used in experimentation are equated in initial performance, and that differences found are related to the variation in the conditions established, though group performances would be expected to vary within limits. An analysis of the means and standard deviations in all conditions is shown in Table 4. A comparison of the measures analyzing original learning scores would suggest that there are group differences in initial performance as shown by a range of means from 64.96 to 73.95, and standard deviations ranging from 10.2 to 15.87

Condition 0 doubtless contained an unusual group of subjects for an analysis of $\frac{D}{PE_D}$ of the five conditions shows a significant difference for Condition 0 when it is compared with Conditions 2, 3, or 4 in original learning (see Table 5).

Because the comparison of averages showed significant differences between the groups in the original learning, a group comparison

TABLE 4

Means and Standard Deviations of the Symbols Completed in Each Test

Condition	A-Test		Interpolation		Relearning of A	
	Mean	*S.D.*	Mean	*S.D.*	Mean	*S.D.*
X	72.85	12.49 ± 1.15			93.15	14.49 ± 1.28
0	73.95	13.23 ± .94	75.10	15.13 ± 1.08	92.43	19.29 ± 1.42
2	64.96	15.87 ± 1.55	68.83	14.28 ± 1.42	82.00	17.58 ± 1.69
3	66.90	10.93 ± .74	81.16	11.09 ± .71	90.12	15.48 ± 1.15
4	68.27	10.20 ± .74	73.34	10.68 ± .71	91.00	12.21 ± .88

TABLE 5

TO SHOW COMPARISONS OF THE AVERAGES IN ORIGINAL LEARNING IN THE FIVE CONDITIONS

Tests compared	PE_D	Diff. in averages	$\frac{D}{PE_D}$	Chances in 100
X with 0	2.1	—1.10	.05	51
X with 2	2.7	7.39	2.9	97
X with 3	2.0	5.95	2.97	98
X with 4	2.0	4.58	2.3	94
0 with 2	2.6	8.99	3.5	99
0 with 3	1.8	7.05	4.0	100
0 with 4	1.7	5.68	3.3	99
2 with 3	2.5	—1.94	.8	70
2 with 4	2.4	—3.31	1.4	83
3 with 4	1.5	—1.37	.9	73

was made on the basis of College Ability Tests, to find out if there were the same group differences as was indicated by rating in the original learning.[4] These scores were available for most of the subjects. Comparison of the group means and standard deviations in the College Ability Tests revealed group differences comparable to the differences of averages in the original learning test of our experiment, i.e., Condition X, a mean of 64.6; Condition 0, 69.4; Condition 2, 59.0; Condition 3, 61.8; and Condition 4, 61.0—sufficient evidence that the groups were not well equated.

But the comparison of results on the basis of original group differences should not radically affect the results summarized in Figure 1, except in so far as different levels of ability may be more affected by retroaction.

It is quite apparent from our results that with Conditions 0 to 4 we have not verified the theory that *"As similarity between interpolation and original memorization is reduced from near identity, retention falls away to a minimum and then rises again, but with decreasing similarity it never reaches the level obtained with maximum similarity"* (8, p. 299). We find no inversion with greater similarity though we have attempted to establish conditions such as Robinson describes.

---

[4] College Ability Tests are entrance examinations given to all freshmen at the University of Minnesota. The rating obtained in this test is considered as one factor in determining college eligibility.

The results in these conditions (Conditions 0 to 4) show the closest agreement with Robinson's latest study in retroaction (8) in which he found a conspicuous absence of inversion with increasing similarity between original learning and interpolated material.

Cheng's (1) comparison of the relative effect of the utilization of different methods of measuring retention, anticipatory recall and relearning, would suggest that in measuring results by the method of saving no inversion is to be expected, although it has been found in measuring the results with the method of recall. Yet, our results agree with Robinson's in the ultimate absence of inversion with increasing similarity, though he measured his results by recall and we measured ours by the method of saving. Therefore, it is not in the method of measurement that the explanation of our failure to demonstrate the typical curve lies.

On the other hand, we have not reproduced all of Robinson's conditions, for we failed to establish a condition of similarity with one element common to the original learning and the interpolation. This condition (between 0 and 2) might have resulted in a demonstration of negative inhibition or negative retroaction greater than that in Condition 0 or 2, and the inversion which is suggested.

But, let us turn again to Condition X. Now, if Condition X is looked upon as "only another variety" of similarity, we may claim to have verified the Robinson-Skaggs Law. Like Harden (3) we can demonstrate the inversion only when the ends of the base line are representative of two different variables. There is a question whether stages from similarity to maximum dissimilarity can be found which would involve only one variable.

General Summary and Conclusions

This study was planned to investigate the comparative retroactive effects as shown by recall after interpolation of several degrees of similarity and recall after "rest."

One hundred and seventy-nine subjects were used. Classes in general psychology laboratory afforded divisions for the five experimental conditions, though the groups differed slightly in size.

In considering Conditions 0 to 4 (see Figure 1) we find a steady increase in saving with increase between similarity of original learning and interpolated material. Without reference to Condition X, we would interpret our results as substantiating the most recent findings of Robinson, for the theoretical inversion is not demonstrated.

Our experiment has been limited by the particular kind of material utilized (letters of the alphabet) and the number of experimental groups of relatively the same constituency available for experimentation. If the theoretical inversion were demonstrable within the more obvious degrees of similarity (Conditions 0 to 4), it might occur in the degree of similarity (one element common to original learning and interpolation) between Conditions 0 and 2. We have no data on this condition. If we considered that the variation from X to 0 were of the same general type as that from 0 to 4, then we might say that our complete function is a verification of the Robinson-Skaggs law.

II. Susceptibility to Retroaction at Various Grade Levels

In the previous study, an attempt was made to analyze some characteristics of this factor of retroaction by comparing the saving scores of each subject with honor point ratios, Army Alpha scores, and College Ability ratings. No significant correlations were found.

Plans were made to study retroactive effects in children at different grade levels to determine the importance of *age* as a differentiating factor in susceptibility to retroaction. An opportunity was afforded in the spring of 1930 to use the children of all grades of the Robert Fulton School of Minneapolis as subjects for experimentation.

Specifically stated, the problem for investigation was to determine the susceptibility to retroaction at various grade levels. The technique utilized in the previous study was considered sufficiently reliable for further investigation, and, because only one condition was to be employed throughout the various grades, the condition which had shown the greatest amount of negative retroaction was chosen for the instrument of measurement. That was Condition 0, the one in which the series consisted of original learning, interpolation of material in which all the elements were comparatively dissimilar, and relearning of the original material.

As stated above, the subjects were the pupils in an elementary school system. They ranged from Grades 2A through 8A. Though a larger number of pupils were included in the test group, the scores of 672 pupils were utilized in the analysis of the problem for reasons to be explained later.

Procedure. With the lower grades, demonstration of the procedure in learning to substitute symbols was much slower than had been the case with college students as subjects; therefore, no attempt was made to maintain a constant period between administration of the tests (three minutes in the previous experiment), but the instructions were adhered to as closely as possible so that conditions would be comparable in the various grades.

It might be assumed in advance that this procedure would be more difficult for the young children in grades as low as the second to comprehend; but it was decided to leave the determination of the adequacy or inadequacy of the technique to the interpretation of results. No final line of demarcation between those who would and those who would not comprehend could be made in advance, and it was assumed that the instructions were sufficiently simple to be clear to the majority of the subjects. Therefore, in the selection of the tests for analysis, all those papers were discarded in which the subject had not substituted at least five symbols correctly. In these cases, it was assumed that he had not comprehended the learning procedure and the paper could be of no value.

Results and Discussion. A final analysis of these data will not be made for some time; so the following is submitted as a preliminary report of this investigation. A sample study of errors was made which showed them to be relatively insignificant; but a complete study will be made for grade comparisons. Analyses of age, errors, and other factors may add to the findings, but are not likely to vitiate any of the results reported here.

The first step in the evaluation of results was the comparison of the average number of symbols completed in each grade. The gradual increase of symbols completed from grade to grade in original learning, interpolation, and relearning is evident, also the superiority of the A group to the B group in each grade in original learning score. (See Table 6 and Figure 3.) Pro-active influence as measured by difference between original and interpolated learning is almost negligible and percentage of saving is relatively constant. The range of saving (as shown in Table 6 and Figure 3) is from 16 to 32%, with no marked variation from Grades 3A to 8A.

The exceptionally large saving in Grades 2A and 3B might indicate a pronounced facilitative effect of the interpolation, but the relatively large pro-active percentages suggest that the learning procedure had not been comprehended by a relatively larger number of the subjects in these grades until the interpolated test had been introduced. If this were the case, even though negative retroaction were operating, the effect would be counterbalanced by this factor

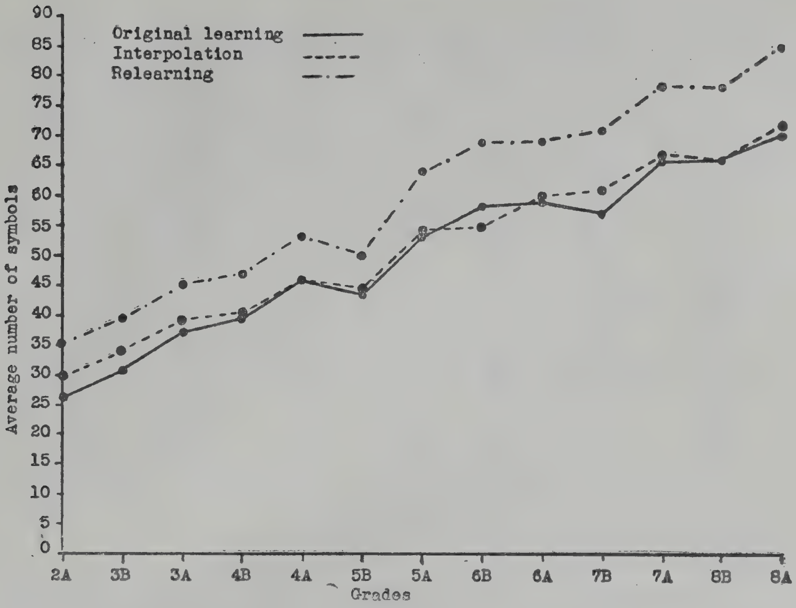


FIGURE 2

Showing the Average Number of Symbols Completed in Grades 2A to 8A in Original Learning, Interpolation, and Relearning

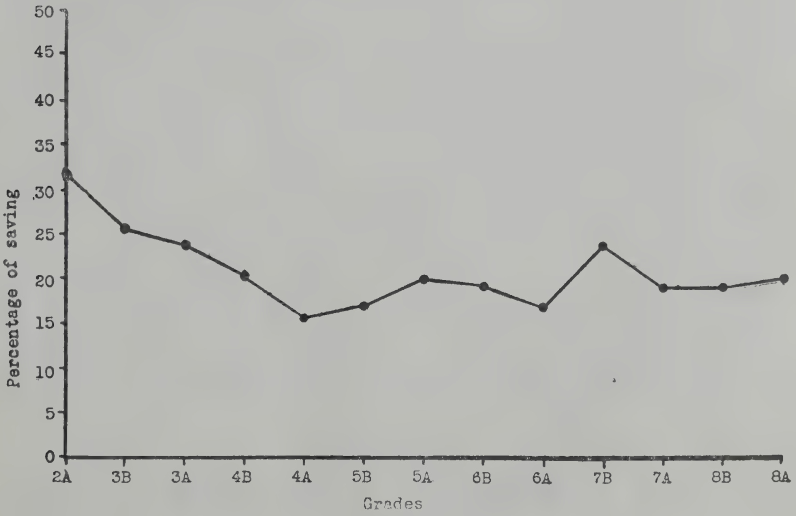


FIGURE 3

Showing the Percentage of Saving for Each Grade in Relearning

TABLE 6

AVERAGE OF SYMBOLS COMPLETED IN EACH TEST IN EACH GRADE; PERCENTAGE OF PRO-ACTIVE INFLUENCE AND PERCENTAGE OF SAVING

Grade	No. of subjects	Av. in original learning	Av. in inter-polation	Av. in relearning	Percentage of pro-active influence	Percentage of final saving
2A	52	27	30	35	.13	.32
3B	20	31	34	39	.09	.26
3A	58	37	39	45	.06	.24
4B	38	39	40	47	.01	.21
4A	67	46	46	53	.01	.16
5B	19	43	44	50	.02	.17
5A	66	53	54	64	.02	.20
6B	34	58	55	69	—.04	.19
6A	65	59	60	69	.03	.17
7B	60	57	61	71	.06	.24
7A	65	66	67	78	.02	.19
8B	54	66	66	78	.00	.19
8A	74	70	72	85	.02	.20

and the saving score would include an unusual increment due to practice.

In these lower grades, too, many papers had been discarded because of evident lack of comprehension of the instructions of the experiment; so, from our preliminary analysis of the data, we may offer no conclusions which would differentiate these grades from others in susceptibility to retroaction. Further work is being considered at these lower levels with a more simplified technique.

Slight irregularities are evident in the low percentages of saving in Grades 4A, 5B, and 6A with a substantial increase in 7A. If these fluctuations were to hold throughout any considerable number of contiguous grades, we would be tempted to attribute them to higher or lower susceptibility to retroaction; but, occurring as they do in discrete order, we may offer no such conclusions.

Therefore, from a preliminary analysis of these data, we may conclude that, in general (except for Grades 2A and 3B which we cannot claim to have measured adequately), there is relatively the same degree of susceptibility to retroaction in all grades included in our study; and there is no apparent increase or decrease with age as indicated by grade location.

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DEUX ÉTUDES DE RÉTROACTION

(Résumé)

On a déjà montré que le degré de similarité entre la matière originale à apprendre et la matière interpolée est un facteur important du rappel. Dans cette étude on a essayé de mesurer l'influence sur le rappel d'une similarité entre la matière originale apprise et celle interpolée et de comparer le rappel après une interpolation similaire avec le rappel après le repos. On a établi des conditions à intervalles égaux pour l'apprentissage original, l'interpolation, et un deuxième apprentissage. L'efficacité du rappel a été mesurée par le pourcentage de l'économie. On a établi cinq conditions et les a utilisées avec cinq différents groupes de sujets. Condition X avec une période interpolée de repos, Condition O avec une interpolation de 0 éléments identiques avec ceux de la matière originale à apprendre, Conditions 2, 3, 4, avec 2, 3, 4 éléments respectivement communs à la matière originale et à celle interpolée. L'instrument de mesure a été un test de substitution fait en employant les symboles géométriques Binet-Simon en sens inverse (A pour Z). On a fait des copies miméographiées pour tester un groupe à la fois.

Les résultats indiquent ce qui suit: (1) Dans les Conditions 0 à 4, qui sont des conditions de similarité augmentante, l'économie augmente progressivement et ne montre aucune inversion avec une similarité augmentante entre la matière originale et celle interpolée. (2) Si, d'autre part, la variation de Condition X à 0 est considérée du même type général comme celle de Condition 0 à 4, une inversion se montre, bien que, comme dans les études précédentes, la ligne de la base soit réprésentative de plus d'une variable.

On a fait la deuxième étude pour tester la sensibilité des élèves de l'école élémentaire à la rétroaction à divers niveaux d'années scolaires

(Classes 2A à 8A). On a employé la même technique comme dans la Condition 0 de la première étude. Les résultats, comme montrés par les chiffres comparatifs de l'économie (voir la Figure 3) indiquent qu'il y a relativement le même degré de sensibilité à la rétroaction à toutes les années scolaires, et qu'il n'existe nulle augmentation ni diminution apparente avec l'âge comme indiqué par l'année scolaire.

Dreis

ZWEI UNTERSUCHUNGEN DER RÜCKWIRKUNG

(Referat)

Der Grad der Änlichkeit zwischen ursprünglichem Lernstoff und interpoliertem Stoff hat sich als eine wichtige Einwirkung auf die Wiederholung erwiesen. In dieser Untersuchung hat man versucht, die Einwirkung der Änlichkeit zwischen dem ursprünglichen und dem interpolierten Lernstoff auf die Wiederholung zu messen und die Wiederholung nach Interpolierung von ähnlichem Stoff mit der Wiederholung nach einer Ruheperiode zu vergleichen. Es wurden Versuchsbedingungen errichtet unter denen die Intervalle für ursprüngliches Lernen, Interpolierung, und Wiederlernen (relearning) gleich waren. Die Tüchtigkeit der Wiederholung wurde an dem Prozent der Ersparung (saving) gemessen. Es wurden fünf Bedingungen (conditions) eingerichtet und an fünf verschiedenen Gruppen von Versuchspersonen angewendet. Unter der Bedingung X gab es eine interpolierte Ruheperiode. Unter Bedingung 0 wurden 0 Bestandteile (elements) interpoliert, die mit Bestandteilen des ursprünglichen Lernstoffs identisch waren. Unter den Bedingungen 2, 3, 4, hatte der interpolierte Lernstoff respektiv 2, 3, und 4 Bestandteile mit dem ursprünglichen Lernstoff gemein. Der Massstab bestand aus einem Substituierungstest der durch die Anwendung der geometrischen Symbole von Binet und Simon in umgekehrter Anordnung konstruiert wurde. Es wurden mimeographierte Abdrücke gemacht, damit eine ganze Gruppe zur selben Zeit geprüft werden konnte.

Unsere Befunde weisen auf folgende Schlüsse hin: (1) Unter den Bedingen 0 bis 4, die Bedingungen zunehmender Änlichkeit darstellen, nimmt die Ersparung fortlaufend zu, und zeigt mit dem Zunehmen der Änlichkeit zwischen dem ursprünglichen und dem interpolierten Lernstoff keine Inversion. (2) Betrachtete man anderseits die Variierung von der Bedingung X zu der Bedingung 0 als der selben Art angehörend wie die Variierung von der Bedingung 0 bis zu 4, so wird eine Inversion erwiesen, obwohl, wie auch in früheren Untersuchungen, unsere Grundlinie (base line) mehrere Variablen darstellt.

Die zweite Untersuchung wurde unternommen, um die Empfindlichkeit (susceptibility) von Elementarschulkindern aus verschiedenen Schulklassen (von der ersten bis zur achten) für die Rückwirkung zu prüfen. Es wurde das selbe Verfahren verwendet, wie unter der Bedingung 0 in der oben beschriebenen Untersuchung. Die Befunde—als Zahlen ausgedrückt, die die relativen Ersparnisse darstellen (comparative saving scores)—weisen darauf hin (siehe Abbildung 3) dass in allen Schulklassen eine relativ gleiche Empfindlichkeit für die Rückwirkung besteht, und dass diese Empfindlichkeit augenscheinlich mit zunehmendem Alter—durch die Schulklasse angedeutet—weder zunimmt noch abnimmt.

Dreis

LEARNING FUNCTIONS IN AN OBSCURE AMNESIA, WITH IMPLICATIONS FOR RE-EDUCATION*

From the Psychology Laboratory of the Boston Psychopathic Hospital

F. L. Wells

Introduction

After dominating educational practice for some generations, the principle of formal discipline passed under a cloud about the first of this century. Experimental evidence accumulated that training in specific functions did not carry over to parallel functions in a manner to justify the previous reliance on formal discipline as an educational principle. Only insofar as the parallel tasks involved identical functions, could a genuine carry-over, or transference, be admitted. Yet it seems largely to have escaped attention that this identity of function is a far from objective thing, and that identities of function might exist for one type or level of mind, where they did not exist for another. It is in accord with this fact, apparently, that the more complex functions one deals with, the more their individual differences increase with practice. The more complex minds can establish more cues to manage the material than can the simpler minds. It follows, accordingly, that practice in any function will irradiate over other functions in accord with their symbolic relation to it, the extent and character of this irradiation being also determined by the particular functioning organism. A Russian boy might copy the text of Shakespeare and get practice only in writing English script; an English boy would also gain acquaintance with a chief monument of language skill and human insight.

Among recent contributions, Pear (12) has been relatively skeptical of transfer effects, while the studies of Beito and Brueckner (2), Casey, Davidson, and Harter (4), Goodenough and Brian (5), Newkirk and Gundlach (10) lend themselves to a more favorable interpretation. Whipple (23) takes an intermediate position, the crux of which, as also with Pear's arguments, is with the meaning of "moods and ideals" (Pear's "abstract sentiments"). The power

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of abstraction is, of course, at one with the power to form cues between the test techniques; and, if this is granted, with its modification by individual differences, there is little further source of disagreement on grounds of principle.

Such considerations as these, together with certain personal experiences involved in the organization of alternative forms of test material, interested the writer in further exploring the possibilities of systematized and concentrated practice as an educational or remedial technique. The present study is in some sense a by-product of a more ambitious program carried out with superior adults in the years 1928-1929, which is still in course of evaluation. A fragment of it has already been reported (20). The present paper indicates the general method and its application in the psychiatric sphere. In general, the procedure is to practice the subject, or patient, to an approximate limit with some standard technique, say Kuhlmann's "Inverted Forms," then discontinue this technique and begin again with some partly analogous technique, as might be Kuhlmann's "Unfamiliar Forms" or the "Inverted Forms" with different line patterns. The forms of the practice curves are to be compared, as well as the test scores themselves, since the essential question is: Does the subject begin the second function at a lower point on the practice curve as a result of his practice on the first function? The test procedures used include various levels of mental functioning, and this feature is essential to the interpretation of findings in this field. Single case studies like the present are necessarily significant rather for methods than results, but the latter also afford encouragement to further development as a psychiatric technique.

The patient was referred to the Laboratory with the following data in the history.[1]

X, a man aged 28, was first seen for psychiatric consultation in July, 1928. Examination by competent internists had revealed no organic disease.

There was a story of a partial amnesia since the experience of a shipwreck six years before. The memory difficulty was of a curious nature, manifested most strikingly in the retention of immediate matters, but spreading back in a diffuse sort of way, as far as the World War period, and in some degree covering the whole life span.

The history was given by his sister and mother. Part of it was derived by them second-hand from various companions of the patient while in the army and on the sea. The family had their permanent home in a small village in Newfoundland. The son was said to have been a normal boy, healthy, athletic, sociable, mischievous, genial in disposition, and a fair student. Physically he was mature beyond his years and enlisted in the Canadian Army at the age of sixteen, serving three years overseas, much of the time in active front-line work. His record was good. He was for some time a courier for the company captain, a position which brought him into special danger and put him much on his own initiative. He suffered no important wounds or illnesses and appeared in normal condition after discharge.

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[1] Notes kindly furnished by Dr. Martin W. Peck.

He had always an ambition to follow the sea and after some apprenticeship as an engineer made two trips on freight vessels, both ending in shipwreck on foreign shores. The first experience brought no ill-effects—the second in 1922, was connected with the onset of his mental symptoms. At the time he was ill with a severe protracted fever (probably typhoid). During a many-day battle with storm and sea, he had been carried below on a stretcher to aid in repairing the pumps. When the men were forced to abandon ship there was some question of leaving him behind as he seemed near death. After a perilous trip in open boats, they reached land, and the unconscious patient was hauled up a cliff by a rope and carried on mule-back to shelter. Many weeks later he arrived home accompanied by some of his shipmates. He was emaciated, weak, and mentally seemed like a child and was confused. He could not recall, for example, that he had a sister in Boston.

Physically he convalesced steadily, but there was less progress mentally. His memory remained impaired and there was a general change in personality in part seemingly reactive to the memory handicap. He was more docile, anxious to please, and sensitive than formerly, and over-grateful for kindness. He felt the need of the protection of living at home. His sociability and sense of humor were retained. He played good football when once on the field, but never could be relied on to remember that a game was scheduled. He helped his father in the blacksmith shop and the latter was irritable and bullied him over his inability to learn simple tasks. At home his mother indulged him in every way. The young man for the most part was optimistic of recovery and held to his ambition of going to sea again. For the few years prior to examination there had been little change in his condition. During this period he made a heroic rescue of a young girl who had fallen from a wharf.

The patient when interviewed was alert, amiable, and responsive, with the formal courtesy of the trained soldier. In spite of memory defects, there was no dullness and he might well be found a good companion.

There was no disturbance of mood or morbid mental content of any order. His memory defect was at once evident but difficult to measure and evaluate. He often filled in the past shrewdly from things he felt must have been so from applications of logic and common sense. He often spoke of "sensing" things as having happened when he could not clearly recall them. Of these indirect means he was wholly aware, as well as when he told of experiences which he learned from others but could never directly remember.

Tests for grasp and retention were poorly done. He spoke humorously of all such things "going through the sieve" of his mind. After several visits to the physician's office with streets and directions carefully pointed out by a companion, he was quite unable to find his way alone. He could not remember the doctor's name, and number or dates of visits, or what had been told him of the nature of his disorder. He was troubled by these failures, but not unduly so. He made much in the beginning of some feeling in his head which interfered with memory and was temporarily relieved when "something snapped." This seemed to be a wholly psychic phenomenon and he was willing to give up, a bit reluctantly, the engineer type of idea that a part had been misplaced in his brain.

A diagnosis of hysterical amnesia was made. The case seemed to warrant a fair prognosis for recovery under appropriate treatment and he was referred to a clinic for intensive psychotherapy.

Initial tests made in the laboratory resulted as follows:

In the special type of memory test developed in the laboratory (18) he showed (8-15-28) no reduction on "Old Personal" items, but his scores on current and common information were reduced to about half normal. Immediate memory for digits was little disturbed; in recognition memory (for postcards) the score was some two-thirds normal average. No other noteworthy disorders were shown, the global point score being some three-fourths average, which, in view of the close relation that subsists between this memory test and Binet score, is probably not far below average for the Binet record described below. The times of individual lines in the Woodworth-Wells Substitution Test are interesting to compare with a normal situation (22). They are, in seconds, 15, 18, 27, 19, 19, 20, 26, 17, 26. There is rather a negative learning effect, or "fatigue."

He was tried (8-15-28) with three formboards, the Kempf Diagonal, the Gwin Triangle, and Healy A(14), each repeated five times. The initial scores were up to adult average. Learning (as shown in reduction of time and moves) would not seem to proceed as fast as is average for adults but was fairly consistent with his observed "intelligence age." Notes on a Stanford-Binet test (16) (8-31-28) decribe the patient as rather tense, at a special disadvantage in timed tests, though on occasion responding very quickly. The record scatters from Year VIII basal (failing Date and Weights at IX) to Year XVI, passing Digits Backwards. The mental age score is 12 years, 5 months, IQ 78, with evidence that it is below the patient's original endowment.

It was then determined to study the learning or practice effects in a number of functions based on fairly standardized psychometric techniques, as follows:

I. *Reversed Clocks.* 5 problems each week

problems changed weekly—Cycles 1-30
first four sets repeated—Cycles 31-34

This test is essentially the same as the reversed clock test in the Stanford revision of the Binet scale. Five new clock problems (16, 19) were presented weekly and the problem was to reverse visually the hands of the clock, i.e., the minute hand for the hour hand and vice versa. The results were scored for accuracy and time. A problem which had for its reversed time absolute accuracy (reversed to the nearest minute) scored four points; one minute off scored three points; two minutes off, two points; three minutes off, one point; four or more minutes off, no credit. For example, 3:36 reversed would be 7:18, scoring four points; 7:17 or 7:19 would score three points, etc. The median time for the five problems was taken as a time score.

II. *Ingenuity.* 3 problems each cycle

Cycles	1-7	3 steps,		3 steps,		5 steps	
"	8-25	3	"	5	"	5	"
"	26-30	5	"	5	"	5	"
"	31-34	5	"	5	"	7	"

These problems are similar to the ingenuity problems in the Stanford revision of the Binet scale (16).

III. *Myers Mental Measure* (9).

Repeated during Cycles 1-25 only

An intelligence test, composed entirely of pictorial material; page 1, ability to follow directions; page 2, supply missing parts; pages 3 and 4, underline similarities. The total score, and the average time for each line on pages 1 and 3 were noted.

IV. *Inverted Forms.*

Set A (Kuhlmann)	Cycles	1-25
Set B	"	26-34

This test is taken from Kuhlmann's revision of the Binet scale (8). It consists of five cards each with a square design having four lines drawn on it. The sets of lines are drawn on a special record sheet. A second series of designs similar to Kuhlmann's was used from Cycles 26 to 34.

V. *Opposites.*

Set A (Kuhlmann) Cycles 1-25
Set B " 26-34

A list of 20 words taken from Kuhlmann's revision of the Binet scale (8, pp. 142-143). The same list was given with the order of the words rearranged each cycle, through Cycle 25. From Cycle 26 to Cycle 34, a list of similar difficulty but with different words was used.

VI. *Addition (Kraepelin).* Cycles 1-34 (22, pp. 42-46)

A test of addition consisting of four vertical rows of numbers. The directions are to add the overlapping pairs for one minute. The number added, and the errors were noted. The test had six different sets of numbers, one thus repeated every six cycles.

VII. *O'Connor 5 (Mechanical Apptitude).* Cycles 1-34

A test devised by Johnson O'Connor (11, pp. 27-34). Three trials were given at each cycle, and the time for each trial recorded.

VIII. *Substitution.* Cycles 1-25

The substitution test devised by Woodworth and Wells (22, pp. 53-55) was used, filling all 10 lines of figures and taking the time by lines. The average time and the time for the first, second, and tenth lines was noted at each trial.

IX and X. *Healy A and Healy B* (3, pp. 112-113)
Cycles 1-19; five times at each cycle
Worcester 2a and 2b (15)
Cycles 20-34: five times at each cycle
" 20-34: five times at each cycle

These formboards were given at each cycle with five trials for each formboard. The time for the first trial, the average time for the remaining trials, the number of moves for the first trial, and the average number of moves for the remaining trials were noted at each cycle.

XI. *Alpha Subtest 8* (*Information*)

Form 8, Cycles 1-18
" 9, 19-34

Subtest 8 of the Army Alpha (24), a test of general information, was administered by the subject's responding orally to each item, a blank sheet of paper covering up subsequent items. To a wrong answer, or to no response, the correct answer was supplied by the examiner after 30 seconds before proceeding to the next item. Correctness and time were noted for each response.

XII. *Free Association* (*Kent-Rosanoff*) (7)

The Kent-Rosanoff list of one hundred stimulus words was used beginning with Cycle 26 and continued through Cycle 34. The subject responded orally to each stimulus word, with the "first word that came into his mind." The test was scored according to the number of highest frequencies, average reaction-time, and other functions.

The procedure was undertaken partly from the standpoint of research and partly from that of re-education, as there was some reason to believe that the exercises involved would be not without value from this standpoint. For summary purposes, the form on which the data were recorded is reproduced, showing the order in which the tests were given in the odd-numbered cycles. In the even-numbered cycles the order was reversed.[2]

Date ....

I. Reversed Clock	Reversed Reading	Time
a.	....	....
b.	....	....
c.	....	....
d.	....	....
e.	....	....

II. Ingenuity
a. and to get *b.* and to get *c.* and to get

III. Myers.	Page	1	2	3	4
	Points				
	Errors				

IV. Inverted Forms

	Time	Errors
a.	....	....
b.	....	....
c.	....	....
d.	....	....
e.	....	....

V. Kuhlmann Opposites Time Errors K

VI. Kraepelin Addition Score Errors

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[2] The bulk of the psychometric work was carried out by Mrs. E. C. Whitman, of the Laboratory Staff.

VII. O'Connor 5 1 2 3 Score

VIII. Substitution 1 2 3 4 5 6 7 8 9 10 Errors

IX. Healy A Time 1 2 3 4 5
Moves 1 2 3 4 5

X. Healy B Time 1 2 3 4 5
Moves 1 2 3 4 5

XI. Alpha Information

Chronologically the work began 9-9-28 and ended 5-27-29. The second cycle came four days after the first, the third five days after the second. They were then repeated each week, excepting that there was a lapse of two weeks between Cycles 8 and 9, 10 and 11, 18 and 19. During each cycle effort was made to get some idea of the patient's conscious memory for the experimental techniques. These notes give as concrete a picture as is available of the memory defects. Brief extracts are as follows:

Cycle 5. Does not recall who examiner was (same examiner for two cycles previously). Repeated instructions necessary for Addition Test. Recalls Alpha Information but thinks questions are different each time; page 4 of Myers test not recalled at all.

Cycle 6. Recalled examiner easily.

Cycle 7. Does not remember who examiner was preceding week; same item (8) on page 3 of Myers test always bothers him. Recalls doing this page and page 4 only after considerable thought; recalls seeing addition test but does not remember how he added. With O'Connor 5, remembers assembling blocks but not the number of trials. Told it was three times, said, "I had it on the tip of my tongue to say three but I thought I was guessing." On Alpha Information not sure whether he underlined correct word or spoke it. On Healy A and B recalled seeing boards but not procedure.

Cycle 10. Recalls easily all tests and their procedure except Inverted Forms and Addition. On Inverted Forms says, "Do I have so long to look at it—no—reversed"; and for the Addition Test says, "Add up each line or across?" After instructions were given began with right-hand column instead of left. Difficulty in recalling procedure on pages 3 and 4 of Myers.

Cycle 12. On Inverted Forms erases lines to make them straighter—no thought of time. Started out very slowly on Addition and then became "time conscious." Pages 2 and 4 of Myers not completed within time limit (first time this has occurred).

Cycle 13. Good memory for all tests and their procedure except Addition. On this he repeats the formula of adding the overlapping pairs but recalls it as being the first and third numbers, then the third and fifth, and so on vertically, which is the nearest he has come to the correct directions.

Cycle 20. Recognized Worcester formboards as new material. Quite sure of this. In the middle of Alpha Information, Form 9, said, "Isn't this a new form?" (second time given).

Cycle 21. Does not remember Worcester formboards, given for the first time the week previously. Recalls previous giving of Form 9 of Alpha Information.

Cycle 22. Remembers doing construction puzzles but does not recall which puzzles, Worcester or Healy.

Cycle 27. First time remembered instructions for Kraepelin Addition but began adding at bottom of right-hand column.

The following is a brief account of what happened in the various experimental functions covered. Those are taken up first in which a particular set of normal data can be cited in comparison.

On the reversed clocks (16, pp. 321-322), in marked contrast to the available records of healthy individuals (a graduate-student group, some 12 in number, no improvement is shown in the accuracy score. The best score was made in Cycle 15, 13 points, the lowest in Cycle 9, when no credits were earned. The standard deviation of the scores is about 5 points.

It is questionable how far the lack of improvement in the accuracy score is a direct function of the memory disorder. It may be partly due to his inability to interpret accuracy in terms more precise than times ending in five or zero. At Cycle 30 further effort was made to get him to understand this. The immediate result was a considerable diminution of both time and accuracy scores; a familiar result of imposing an unaccustomed work pattern, though it was apparently improving when the work had to be discontinued.

The time score shows definite improvement, from a level of around 20 seconds to 10 seconds. This initial time score of 19 seconds is faster than that of the group of healthy adults who average 170 Alpha. He was remarked to be quick on the corresponding test in Stanford-Binet, which he passed readily. There are three days, including the 25th and 29th, when the time is as long as originally, but half of the last 25 days are at 10 seconds or below. There is no perceptible relation between the speed and accuracy scores for the experimental period.

Thus, as compared in this function with a group of individuals of superior mental capacity, the patient showed little if any failure of learning ability from the standpoint of speed. In accuracy there was marked failure of normal improvement though this cannot with certainty be related directly to a learning failure but rather to a "learning" failure at another level, that is, in grasping the problem in its intended sense, in spite of repeated effort at explanation.

In view of clinical data, it was to be expected that the patient would not carry from cycle to cycle the normal degree of memory for the psychometric material or technique. As noted, record was kept for each test for the manner in which it was recalled from cycle to cycle. The Reversed Clock Test shows a stable recall of the task through the entire series.

With the Ingenuity Test (16, pp. 345-348) it is important to recall that the series was always considerably easier than that of the normal comparison group, which consisted of either a five-, a nine-, and an eleven-step problem or a five-, a seven-, and a thirteen-step; each cycle with the normal subjects also consisted of three new problems. With the patient, the problems were not changed at each cycle but in the manner previously noted; that is, the initial problem was changed every cycle to Cycle 26, when it was kept at 4 and 5 to get 3 for the remainder. The second problem was a three-step problem changed at each cycle until Cycle 8, when it was altered to a five-step (5 and 8 to get 2) until Cycle 21, when it became 4 and 5 to get 3 until Cycle 26, when it became 9 and 7 to get 11 for the remainder. The third problem was a five-step (5 and 8 to get 2) until Cycle 8, 7 and 4 to get 10 until Cycle 21, 9 and 7 to get 11 until Cycle 26, after which it was changed at each cycle. The last four cycles used a seven-step problem change at each cycle.

In the initial three-step problems, changed at each cycle, he reached a maximal level of speed on the third day, but this happened only once again during the remaining cycles, the time showing an average of 10 seconds, with slowest performances of 42 and 22 seconds in Cycles 18 and 19, respectively (recall of the task was subjectively normal on these days). His solution (in Cycle 16) of an erroneously introduced eleven-step problem (5 and 8 to get 4) in 291 seconds is significant. The suggestion is not of a stabilized failure of learning so much as of marked fluctuation, usually, indeed, to a minus level, but occasionally carrying well within superior limits.

In this connection it may be noted that in the first cycle he solved the problem 5 and 8 to get 2 in less than 3 minutes; a most unlikely performance for one with an actual "mental age" of 12 years, 5 months. This was his first contact with the ingenuity problems; he had not been given them in his previous Stanford-Binet, having failed all 16-year tests except Digits Backwards.

This problem, 5 and 8 to get 2, was part of every cycle up to Cycle 20. A person who solves it at first sight in 165 seconds should solve it in minimal time thereafter. On the second trial he took 135 seconds, on the third, 342 seconds. Thereafter a rapid decrease occurs, the final time averaging 18 seconds, the shortest 9 seconds (Cycle 15). This average level is about that which a person *practiced in that class of problem* might show on seeing this problem for the first time. The diurnal variation is considerably greater than such a person would show.

The same situation is exaggerated in the five-step problem 7 and 4 to get 10, repeated from Cycle 8 to Cycle 20. On first trial he gave it up, solving it in Cycles 9, 10, and 11 in 167 to 171 and 300 seconds, respectively. At Cycle 11 he spontaneously utilized an improved method of notation only developed in the laboratory after considerable work with the test (20); and thereafter his times for this problem fluctuated between 17 seconds and 62 seconds. Similar fluctuations characterized the other problems; his quickest performance is about what a practiced person would do with it on first contact, the slowest three or four times as long. The four different seven-step problems, which he had on the final four days, he solved in an average of 45 seconds. This is in line with his solving of the accidental eleven-step problem of Cycle 16. There appears a potential level of psychic functioning much above what is usually at the surface.

The learning curve of the more difficult series made with the superior, healthy subjects begins with an average time of 25 seconds *per step of solution,* which by Cycle 4 has dropped to 10 seconds, and from there continues at a decelerating rate to a little less than 3 seconds at Cycle 30. To this the patient's learning is markedly inferior, thus, for the first seven cycles, the third problem being 5 and 8 to get 2, he always solved it but in the following series of times in seconds, 165, 134, 342, 45, 70, 23, 26. At Cycle 3 he expressed no recollection of having done any ingenuity problems before. Yet, although he took very long for this third problem, he did a new three-step problem, 6 and 5 to get 1, almost immediately, while taking 60 seconds for the next, 8 and 3 to get 5. His best time for the three-step problems he had previously had was 9 seconds, an evidence of learning present below the level of what was at the time conscious.

At Cycle 11 his recall again failed, and he failed to solve the problem 7 and 4 to get 10, but on the following cycle he again did it as well as ever. In general, there is a marked reduction of learning capacity in this function, though there is still evidence (as is strikingly the case with normals) of ability to carry over the methods for previous problems to the solution of later ones.

The Kuhlmann-Binet test of drawing inverted forms (8, pp. 147-148) had already shown in the laboratory some learning phenomena of interest, and so was included in this series. As Kuhlmann's scoring formula, which combines time and errors, is awkward for other than age-level purposes, a separate account was here kept of the two. The initial performance was at a *superior adult* level, Kuhlmann score 94 with one error. Errors practically disappeared after Cycle 5, and the times fluctuate rather less than those of the ingenuity problems. As compared with the available norm, which is from the upper 3% of intelligences, the patient's learning curve is about of the same form but the speed is two or three times slower at both start and finish. Initial learning is relatively rapid in both normals and the present subject, though the limits of practice do not appear reached in either case. The patient's Cycle 8 shows a dip to a level of quickness not consistently reached until Cycle 16 except for a similar drop at Cycle 13; again the evidence of potential efficiency overlaid by other factors. It may also be noted that at Cycle 2 there was no conscious memory of this experimental task as performed in Cycle 1; but, in spite of this, the average time of Cycle 2 was but half that of Cycle 1, a greater gain than shown by the normal subjects who might be assumed to have relatively complete memory for the previous experience.

At Cycle 25 in both normal and patient's records the original Kuhlmann drawings were discontinued and others substituted in which the direction of the lines was different. In both cases the substitution of different lines had but slight and transitory effect. Indeed the patient throughout did better on the new lines than on the last cycle with the old ones. The learning pattern carried over unaffected by factors in his memory difficulty.

The three functions already considered, Reversed Clocks, Ingenuity Test, and Inverted Forms, preserve the following relation of the patient to the normal data. The patient learns relatively less in the Reversed Clocks, somewhat more in the Ingenuity, and about as much as the normal in the Inverted Forms. The first two are relatively ideational tests, the third is more of a performance test. The further data may profitably be considered in the light of a hypothesis that the patient's difficulty is one that affects learning at the ideational level rather than at "performance" or motor levels.

The Kuhlmann-Binet series also contains an opposites test (8, pp. 142-143) in which the stimulus words are printed on a card given to the subject with instructions to call off the opposites as rapidly as possible. In the present experiment these 20 words were placed in a different order for each cycle.

The time curve is more consistent in this function and at the close of 25 cycles is at a level about one-third of the initial record (56″:18″): the gain, though at a somewhat slower rate than in the normal data, is quite comparable in amount. The patient's performance is, however, relatively characterized by errors, which are normally absent; and these are not eliminated by practice. These errors are not, of course, matters of ignorance or misinformation; they are false reactions in the technical sense of a psychomotor rather than ideational process, and add another touch to the patchy effect developing in the psychometric picture.

At Cycle 25 a new list of similarly easy opposites was substituted. There was no apparent carry-over, the first and final cycles comparing as 50 and 22 in the new series as against 53 and 22 in the original. (Gain is not more rapid in the later series.) This is in contrast to the normal tendency, where there is marked carry-over to such a new series.

As another simple speed function, an addition test was made after the manner associated with Kraepelin, adding rows of digits in successive overlapping pairs (Woodworth-Wells test, Stoelting blank) (22, pp. 42-46). The task was to add as many pairs as possible in one minute. It is not the rule in this technique to take account of errors but they were here noted and found more numerous with the patient than in the control group where they are practically absent. From the quantitative standpoint the patient's gain is relatively the same as in the healthy group (40:63 as against 57:90) but the gain was still progressing in the normals, while in the patient the limit had apparently been reached. A peak of 69 additions was attained at Cycle 23 but not again attained during the following 11 cycles. In the present normals, too, the limit is rather one of speed in articulation, while in the patient it was apparently fixed at a more strictly mental level. The considerable amount of practice as shown by the patient may reflect the fact that the instruction for this technique did not fully register during the first 26 cycles. Not at any time did he fully recall what he had to do without some further explanation. Indeed, in the 34th and last cycle he began a wrong adding procedure at the bottom of the right-hand column. The practice effects, indubitably present, registered at a lower level than the comprehension of the task.

The "performance" type of tests which was used has already been indicated. Practice curves were taken with tests O'Connor 5 (Wiggly Blocks), Healy A and B, and Woodworth-Wells Substitution. Their chief common factor is their non-verbal character; in their psychological relations they prove to differ widely.

The normal data hitherto cited for comparison are available with O'Connor 5 (11, pp. 27-34) only. The scores here considered are the averages of the three trials at each cycle, without regard to O'Connor's "improvement factors." The available normal average improvement is from an initial score of 3.25′ to a final score (total 20 cycles) of ca. .65′. The patient's initial score is 4.8′ and improves by Cycle 20 to a general level of 1.3′, though four previous scores had gone below 1′. There was subsequent improvement to a general level of .75′. His learning is clearly less, yet is of the same order as that of the superior normals, while that of the ingenuity and reversed-clock accuracy techniques is not. His learning is also slower in this test, and his memory for the task was defective in each of the first seven cycles. Thus during this period he carried no recollection of the number of trials, three. Cycle 7 had the poorest score of his record in this test; but from then on he made rapid improvement, and his conscious retention of the task also appeared normal. It is worth noting that the normal intraserial learning (i.e., gain in the first three trials of the same cycle, represented by O'Connor's initial "improvement factors" of 1.4 and 1.7) is in this test substantially absent with the patient.

In tests Healy A and B (3, pp. 112-113) there are no normal data for direct comparison, though it may be taken for granted that normal adults learn the boards within five or six trials, speed thereafter being a mainly motor accomplishment. The unrepresentative mental age score of 12½ years should put the patient at no special disadvantage for this class of material. Apparently he takes longer to learn it than a comparable normal individual would do, but the limits reached are of the same order. For Healy A the initial performance has the quite inferior score of 90 seconds, which after Cycle 6 is generally below 10 seconds. From the point of view of intraserial learning, the remaining four trials of each cycle are significant in comparison, the scores of each trial in the first three cycles being as follows:

Time	1.	90—12—17—13—12	Moves	1.	19—5—7—5—6
	2.	65—10— 9—11— 8		2.	17—5—5—6—5
	3.	68—14— 5— 9— 7		3.	18—6—5—5—5

During these cycles, he appears to have recalled subjectively the nature of the task, but obviously not its execution. The situation with Healy B is essentially similar though less clear cut. At the first trial he apparently could not complete it, and after 30 moves he was shown. The first trials of Cycles 17-20 were 20 seconds or less. After the third cycle the scores show a very normal learning curve. As to intraserial learning, the successive trials of Cycles 1-3 compare as follows:

Time	1.	*x*—52—240—32—48	Moves	(30)—11—20—11—11
	2.	55—45— 38—31—33		11 —12—11—11—11
	3.	167—47— 29—26—31		24 —13—11—11—13

During this period both formboards show marked immediate learning, which is lost in the interval. Gradually more and more is carried over to the initial trials of succeeding cycles, so that the intraserial learning is leveled out, and the final performances are quite within normal limits.

At Cycle 20 the formboards Healy A and B were discontinued, and their places taken by Worcester formboards 2*a* and 2*b* (15). These seem to have the same order of difficulty as Healy A; markedly easier than Healy B. At the mental level of the patient, as of normal adults generally, all are in the beginning distinctly mental tasks; a pattern of performance has to be learned, after which the task becomes essentially motor, governed by the time it takes to manipulate the pieces is now well-known ways. In the beginning it takes the patient indefinitely longer to do Healy B than A; but at the end only about twice as long, there being twice the number of pieces. In both Healy A and B the pattern of performance appears established by Cycle 4; the gains thereafter are gains in motor skill. In both the Worcester boards the learning curve starts at a point corresponding to the learning of the pattern of performance, and its later course is that of a gradual perfection of motor skill. There is now an ability to handle formboard material generally that did not exist prior to the practice with Healy A and B. The time for *2a* decreases from 30 to 15 seconds; for *2b* from 35 to 18 seconds; they use the same number of pieces. Except in the initial cycle, intraserial gains are insignificant or even negative. In the second and third cycles of the Worcester boards there was no memory for them or their procedure, although gains were recorded. In fact, speeds approaching the greatest reached occurred in the fourth cycle (Cycle 23), and in neither of the tests was there on this occasion the full subjective memory for the tasks that would normally have been acquired.

Not enough is known about the long-section learning properties of the "substitution" test (22, pp. 53-55) for a satisfactory evaluation of the patient's record. The performance is quite slow, even looking on his mental age score as representative. The improvement is also more gradual than usually associated with practice curves in a function like this; after 20 cycles the speed has about doubled (average time per line 22.7:11.4"). The cultural handicap of a vocation little concerned with writing movements must be considered in this test, rather more than with Kuhlmann's "inverted forms." However, it would seem that the patient learned in this test much less readily than in the formboards.

The test is one in which intraserial learning might be expected to play a considerable part; that one would, at first sight, do later lines much more rapidly as one establishes the form-number associations. In the experience available (22) this is but little the case normally, the last nine lines tending to average about 90% as long as the first. In the patient, the tendency is in the other direction. The first line is generally done more rapidly than the average, and this tendency is substantially unaffected by the 25 cycles of practice, which cut the total time about in half. This function, then, appears one in which the patient learns with relative difficulty.

At the second trial he could not remember the task, only having seen the sheet. Afterwards he recalled the task. A special observation should have been made of his recall for the key between cycles.

The observations with Alpha Subtest 8 (24) also suffer from the lack of control data, which in turn are complicated by the foreign origin of the patient, making many items culturally unfamiliar as they are at best out of date. Furthermore, the learning curve for Alpha Subtest 8 (Form 8) cannot be presented in its entirety since the entire subtest was not given until Cycle 8, and it is probable that some answers could have been given prior to their being first recorded. However, of the 40 items comprising the subtest, he got 14 in Form 8 without being told them, and their average time was 11.2 seconds. Of the corresponding items in Form 9, first given after the 18 cycles of practice with Form 8, he got 21 on the first trial and their average time was 7.7 seconds. There is nothing in the verbal content of the two subtest forms to make him so much better informed on that of Form 9 than of Form 8. For our own culture, such a chance is very small, from the way the tests were organized. In a group of 47 cases in whom it was possible to compare an initial performance in Army Alpha Subtest 8 of Forms 8 and 9, one person scored twice as much and one person 1½ times as much on Subtest 8 of Form 9 as on Subtest 8 of Form 8. One person also scored 1½ times as much on Subtest 8 of Form 8 as on Subtest 8 of Form 9. (In these 47 cases, the entire Army Alpha Form 9 had been given first, and Form 8 immediately afterwards.) There is thus considerable evidence that the re-educative object of the work was insofar attained that a level of accomplishment in this class of material had been raised from one approximating a general percentile score of ca. 55 to one of ca. 77. This is in accord with the previous suspicion that the intellectual level is not a little higher than represented by the Stanford-Binet mental age score of 12½ years.

The time of producing responses in this test was recorded, and, from the standpoint of conscious levels, some interest attaches to these. In accord with directions, the patient's general tendency was to give a response, even if guessed, rather than let it go by default. Time of right and wrong responses can thus also be compared, not forgetting, of course, the likelihood that one-fourth of the correct responses are so by chance alone.

In the time of correct responses for the 20 cycles of Form 8, there appears no change. Its median is 5.0", which is, of course, abnormally long; the sort of reaction associated with the traditional conception of "thinking difficulty." The items are known, but they take this unusual time to "dig up" to the level of expression. (Cf. also the examiner's comment on the "timed" tests of the Stanford-Binet.) The long periods of time which can thus be consumed are worth recognizing. They are meant to be observed through Item 23 of the memory test already mentioned (19, pp. 170-191) but this had been here omitted, as culturally unadapted to a foreign patient. In Form 8, nearly 10% of the correct responses are longer than 15 seconds, and 5 were recorded after 30 seconds. In the subsequent Form 9, about 6.5% were longer than 15 seconds, and 2 were recorded of 30 seconds. Another phase of this matter is the fluctuation between ability and inability to produce the correct response for particular items. It is frequent for an item to be recalled on one day and not the next, again the next, etc. Thus the course of Item 5 in Form 8 (Sapphires are usually blue—red—green—yellow) is as follows, a plain figure representing time in seconds for correct responses, italics time for a wrong response, *X* no response produced for thirty seconds: 11-X-*10*-8-*6*-*4*-10-5-5-*9*-9-6-6-*28*-*12*-12-2-12.

Some items appeared next to impossible for the patient to learn (question of analytic significance?), thus Item 11, Form 8 (artichoke), was answered correctly only twice in the 18 cycles, in Cycles 8 and 18. Item 7 in Form 9 (Irving Cobb, *sic*) was never answered correctly, through the 11 cycles. The incorrect responses are considerably longer than the correct, medians being 10.5 seconds in Form 8 and 8.5 seconds in Form 9. They averaged about 40% of all responses made, and were relatively more numerous in Form 9, which, despite its "flying start" apparently derived from the practice with Form 8, never yielded quite as high scores as its predecessor.

The Myers Mental Measure (9) was used for a period of 25 cycles, with this departure from the standard procedure, that for the first and third pages the patient was allowed as much time as he wished, this being recorded. The scores are therefore higher than the Myers norms, and not comparable thereto. Under these conditions the initial and final scores for the four pages, and their totals, are as follows:

Page	1	2	3	4	Total
1st	22	9	9	13	53
25th	43	24	10	32	109

The scores are substantially doubled in each case. The question is: To what extent is this re-education, to what extent education? In the absence of normal data on practiceability with the Myers technique only a very partial discussion can be given. However, the initial Myers score is 53. This is somewhat above (roughly equivalent to one and one-half years of mental age) the Stanford-Binet score, which, aside from inherent unreliabilities, might be accounted for by the non-enforcement of time limits on pages 1 and 3. There is a very large gain except on the third page, in which the total score possible is 10 points. Along with this is a general, though irregular, decrease in the time consumed on pages 1 and 3 ranging from 2 to 6 seconds per *average* item. Chief attention may, however, be directed to pages 2 and 4 which were given according to Myers' rules. These items are essentially concerned with "altitude" (17, pp. 24 ff.) or "power" rather than "speed," although global time limits are set. It is necessary to study the record in the detail of the individual items. The evidence of such scrutiny is rather that his slowness prevented him from reaching the more difficult items in the earlier cycles, rather than that he could not do them. As practice enabled him to accomplish the earlier items more quickly, there was time for the later ones. There is no clear evidence of an item originally impossible, and learned as a result of the test. The essential gain is in speed, not in "power."

In the items of the first page, where as much time as wanted was allowed, there is some indication of a gain in "power," there being a gradual attainment of mastery over the last three items. The 11th item is acquired first by a considerable margin, and then the 10th. The 12th was done correctly only in the last cycle, its score being perfect for this page.

In the analogous effect with Alpha Subtest 8, one may more readily look upon the process as re-education, since there is no way in which the acquisition of 20 or 30 isolated items of information will impart the knowledge of 20 or 30 equally isolated other items. That information seems to have been present, but buried. The exercises effect a stirring-up of the mental soil which either brings the associations nearer the surface, or makes them easier to reach. The absence of alternate material for the Myers test makes the interpretation less certain in its case.

During the last nine cycles, 26-34, a Kent-Rosanoff (7) Free Association Test was included either at the beginning or end of the test series. From the little now known about practice effects in this function it may be expected that the normal tendency would be to decrease the time of response, and possibly to give them a higher "commonality." The reaction-time does decrease markedly, from an average of nearly 3 seconds to less than 1 second. Four rather comparable subjects observed by the writer several years ago (18) started about the same and ended at a level approximating 2 seconds. But in their case the stimulus words were changed from series to series; on the other hand, they were healthy individuals. It is thus not clear whether this decrease in the patient can be interpreted as a general gain in facility with that sort of material, as in the normal cases it undoubtedly can be. It would seem that the question should be discussed on the basis of responses that were altered through the various series. We do not know the normal tendency to do this, save as reflected in the "reproduction" technique of Jung and his pupils (6). It may be remarked, however, that with weekly repetitions the general gain in reaction-time is definitely greater in this patient than in the normal average reported by Pfenninger (13). Aptekmann (1) notes that the reaction-time gains much faster and farther if the repetition is daily, just as the percentage of changed responses is smaller under these conditions. Two of the sources cited by Aptekmann agree that both changed and unchanged responses participate in the practice gain; in the third (Jung and Brill) the changed responses even lose somewhat, and the repeated ones gain only slightly. For the present data, a study is made of those stimulus words, 10 in number, which showed five or more different response words. The average time of their responses through the successive series runs as follows in seconds:

3.8—4.3—2.4—2.1—1.6—1.9—1.9—1.5—1.1

This gain is rather more than in those words where the response was less changed. As a corollary, the specially long initial times of these changeable responses may be noted, as it was also pointed out by Aptekmann. The average time of all these changeable responses, *irrespective of the series in which they occurred,* is 2.4 seconds. The average time of that portion of them which was the same as the response in the preceding series, 21 in number, is 2.1 seconds. Three stimulus words did not produce the same response in any succeeding series. The course of their response times is:

beautiful	2.6—9.0—1.8—3.8—2.0—1.6—1.6—2.8—1.2
light	10.0—3.2—5.8—3.8— .8—3.2—1.2—1.6— .8
long	1.8—3.0—1.2—1.8—1.2—2.8—1.2—1.8—1.2

A generalized facilitation of response to this test, independent of repeated responses, is indicated. The gross gain is somewhat larger than that reported for normal subjects in the cited Jungian work.

CONCLUSION

The data may now be summarized from the two chief points of reference: (*a*) learning ability as related to the norm and to the character of the functions concerned (*b*) evidence of transfer, that is, a favorable effect of improvement in one accomplishment, upon other, more or less allied accomplishments.

In reversed-clock accuracy his gain is nil, while the norm is considerable. In speed for this function the patient's gain is marked, though not as much as the norm.

The ingenuity test shows some learning, but the performance is always at a much lower level than the available norm.

The inverted forms appear well within normal limits both for general level and learning capacity.

The opposites test shows phenomena essentially similar to the inverted forms; generally slower than the available learning curve, though hardly so as compared to a general average. A similar situation appears in the addition test. Again in the O'Connor 5 test, though it is superficially quite different in nature, the same general learning situation prevails.

For the remaining tests the direct comparison with (superior) normals is unavailable, but the facts essential for comparison seem sufficiently established otherwise.

In the formboards the learning is slower with regard to time, but good in regard to moves, save that in the beginning there was little or no retention from cycle to cycle. The limits reached are about what they would be normally.

Learning in the substitution test appears rather less than normal, much less than in most other functions studied. The information items of the Alpha test are apparently also learned—or relearned—with relative difficulty.

The gains in the Myers test are distinct, but, as with reversed clock, appear to be functions of speed, and certainly much less than would be shown by normal individuals under similar circumstances.

The only really measurable gain in the association test—that in speed—is apparently of a quite normal order.

For synoptic purposes the functions studied may be divided into two groups: (*a*) those which show about the normal order of learning, (*b*) those in which the learning capacity appears reduced. They group themselves as follows:

a	*b*
Reversed clock (speed)	Reversed clock (accuracy)
Inverted forms	Ingenuity
Opposites	Substitution
Addition	Alpha information
O'Connor 5	Myers
Healy A	
Healy B	
Association time	

In seeking to formulate distinguishing features of these two groups of functions, one might first consider that the *a* group emphasizes quantitative functions, particularly speed, while in the *b* group qualitative features have more prominence. In *a* group the ideational processes involved are simpler, the motor and kinaesthetic ones of greater importance. In general, those of *a* are more favorable to "learning as doing," and those of *b* to "learning as a reasoning." Anything that produced a restriction of the field of consciousness, or a difficulty of attentional control, would be likely to affect the functions of *b* more than those of *a*.

The question of transfer is of relative importance in this case, as it carries most of the implications for re-education. Some directly comparative data from healthy individuals are again available. Nothing of importance appears from this standpoint in regard to the reversed-clock test. Normal individuals show marked improvement in the ingenuity test, carried over to continuous changes of problem, but nothing of the same order appears with the patient. In the inverted forms, on the other hand, the transfer effect to changed-line directions was practically the normal, which is great. The normal carry-over was again absent in the opposites test with the patient, though the fact that these experimental conditions do normally show transfer is significant from a general standpoint. Nothing in the nature of transfer effects, as ordinarily conceived, is involved in the present addition test, and the same may be said of O'Connor 5.

In the formboards, on the other hand, the transfer phenomena are quite marked, as from Healy A and B to Worcester 2*a* and 2*b*. The practice has undoubtedly made the patient better at formboards in general. How much better, and at what other things in general, is the heart of the whole problem of formal discipline.

The carry-over in both speed and "power" from Form 8 to Form 9 of Alpha Information seems striking among the transfer effects indicated. It is probably conditioned by the clinical situation, and would hardly be duplicated in normal subjects. The evidence of increasing readiness, with practice, in meeting the free association test situation, which is normal, is the only remaining phenomenon of this nature in the material.

The patient was under psychiatric treatment[3] during the entire time of these observations, and later. No marked improvement in general condition occurred during the experimental period. The relative significance of the experimental work for such improvement as there was cannot be estimated. Under subsequent psychiatric treatment the patient achieved marked improvement, being able to resume his former occupation. The behavior of the learning curves according to the levels of the functions involved rather bears out the interpretation put upon the case psychiatrically, of unconscious factors leading to a failure of the normal registration of external impressions. The measureable diffusion of improvement among functions nearly related to those exercised is such as to encourage further activity along these lines. As compared with accepted occupational therapy, it is more disciplined, which is not necessarily an advantage, but progress in the functions exercised is perhaps more readily appreciated by the patient, owing to its more quantitative nature. A further trial of the procedure as an adjunct to the recent advances in the treatment of general paresis is perhaps especially suggested.

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[3] For information concerning the patient's status during and after this experimental work and for critical comments on this account of it, acknowledgment is due to Dr. Henry A. Murray of the Harvard Psychological Clinic, who had psychiatric charge of the patient.

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LES FONCTIONS DE L'APPRENTISSAGE DANS UNE AMNÉSIE OBSCURE, AVEC DES SUGGESTIONS POUR LA RÉEDUCATION

(Résumé)

Pendant à peu près neuf mois on a étudié un sujet affligé d'une amnésie dont la base a été incertaine à ce temps, en employant un processus basé sur la répétition systématique de plusieurs tests étalonnés comme exercices pratiques. On a adapté les tests des séries fréquemment employées de tests Binet et de tests d'exécution. Après avoir atteint les limites approximées de l'exercice sur ces fonctions, on a commencé l'exercice avec des processus en rapport, pour voir comment le progrès de ce deuxième exercice pourrait être influencé par l'exercice précédent; il a été aussi possible de

comparer le progrès du premier exercice avec celui des adultes supérieurs en bonne santé lesquels ont subi des exercices semblables. A ce propos, le malade a montré, vis-à-vis des personnes en bonne santé, une plus grande diminution de l'effet d'exercice dans ces fonctions qui dépendent des processus d'idéation plutôt que des processus moteurs. Dans certaines fonctions, c'est-à-dire, les planchettes ("formboards"), Army Alpha test secondaire 8 et le test d'association libre (Kent-Rosanoff), il s'est montré une plus grande facilité générale avec le type des matières impliquées, un effet de transfert dans le sens de discipline "formelle." Le caractère des difficultés trouvées dans l'apprentissage semble se prêter à une interprétation fonctionnelle plutôt qu'organique, de même que le progrès subséquent de l'amnésie sous la direction psychiatrique.

WELLS

DIE LERNFUNKTIONEN IN EINEM SCHWER-DEUTBAREN AMNESIEFALL, SAMT ANWEISUNGEN FÜR DIE NEUERZIEHUNG

(Referat)

Man untersuchte während ungefähr neun Monaten einen Patient, der an einer Gedächtnisstörung lit, deren Basis damals noch ungewiss war. Man gebrauchte ein Verfahren, das sich auf die systematische Wiederholung einer Zahl klassischer (standard) Tests als Übungungen stützte. Die Tests waren grossenteils Anpassungen von der Binet Serie und von Ausführungsprüfungen (performance tests). Nachdem die Grenzen der Besserung dieser Funktionen durch Übung annähernd erreicht worden waren, wurde Übung mit verwandten Tätigkeiten angefangen, um zu bestimmen, wie der Lauf dieser späteren Übung durch die vorhergehende Übung beeinflusst sein würde. Man konnte ebenfalls den Lauf der ursprünglichen Übung bei dem Patienten mit dem Lauf bei überlegenen (superior) Erwachsenen, die ähnliche Übungen durchgemacht hatten, vergleichen. In letzterer Beziehung erwies der Patient im Vergleich mit normalen Personen eine stärkere Verminderung der Wirkung der Einübung bei Funktionen, die von Gedankenvorgängen (ideational processes) abhängig waren, als bei motorisch bestimmten Vorgängen. In Bezug auf gewisse Funktionen, i.e., die Formbretter (form boards), den Army Alpha Prüfungsteil 8 (Subtest 8) und die freie Assoziationsprüfung (free association test) (Kent-Rosanoff), zeigte sich eine Zunahme an allgemeinen Geläufigkeit mit der in Anspruch genommenen Stoffart (class of material), also eine Übertragungswirkung (transfer effect) im Sinne der "formellen" Diziplin. Die Art der entdeckten Lernschwierigkeiten, wie auch der nachfolgende Lauf der Störung unter psychiatrischer Behandlung scheint eher mit einer funktionellen als mit einer organischen Deutung übereinzustimmen.

WELLS

ON THE NATURE OF "SPEED" AND ITS RELATION TO OTHER VARIABLES*

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Most experimental investigations on the nature of "speed" or rate of performance have sought to determine the relation between speed and another variable termed "altitude," "power," "intelligence," "mental ability," "ability," "quality," "goodness of response," and so on. These terms, however, have rarely been defined with precision, and in experimental studies the functions under investigation have rarely been unequivocally isolated. As a result, there is considerable controversy regarding the rôle of speed in the "higher" mental functions. The present paper employs the term "altitude" in preference to the others listed above, since it carries no connotation of speed [see (15, p. 22)]; an individual's altitude score on a particular type of task is defined as the difficulty level he can reach in a series of problems of increasing difficulty when no time restriction is made.

Rate of performance, or the speed variable, is usually defined or circumscribed either in terms of the amount of time necessary to make a response or complete a task, or in terms of the number of responses or tasks of equal difficulty performed in a given interval of time. These responses or tasks have varied all the way from simple reaction-time to complex mental tasks. Some of the existing confusion regarding the speed-altitude relation has arisen because investigators have ignored the fact that their speed scores were in terms of rate of performance on tasks of unequal difficulty.

THEORIES CONCERNING THE RELATION OF SPEED AND ALTITUDE

Theory 1. There is no community of function between the two abilities. This theory implies *different* physiological counterparts for the two abilities, for example, altitude ability being determined

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by the number of available connections,[1] and speed being determined by the speed of nerve conduction.

Theory 2. There is some community of function between the two abilities. This theory implies dependence of the two abilities on a *common* physiological basis. Lemmon (8) offers the theory that speed of nerve conduction itself may be a causal factor in altitude ability and hence would account for any correlation between the two. He says:

> "It is possible that the quality of intelligence may depend not only upon the number of connections, but also upon the speed with which these connections are formed. Nerve centers cannot remain excited indefinitely at maximum intensity; consesequently in the case of a person who forms connections slowly, it is possible that the excitation of the first association centers to be affected will have diminished or disappeared before the later ones come into play. Thus only a limited number of centers are cooperating at any one time. The person who forms connections quickly, however, is apt to have more association centers interacting at once since the later centers are aroused before the earlier ones have had a chance to lose their effectiveness."

The converse of this reasoning offers an equally good hypothesis to account for the correlation between speed and altitude on hypothetical physiological grounds. Instead of speed of connection facilitating altitude ability, it is also plausible to assume that altitude causally facilitates speed of response. If altitude of intellectual ability is considered to hinge upon number of connections available, we may suppose that the individual with the most connections would have available a shorter neural route for the completion of an in-

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[1] For purposes of this discussion of theoretical physiological counterparts of "altitude," we are using Thorndike's terminology. But Lashley (7) (1929), espousing a dynamical theory, and Thorndike (15) (1927) favoring the aggregate theory of intelligence would seem to agree that *quantity* of cortical tissue is important in determining the *quality* of intelligence. Thorndike says (15, p. 415), "Let *c* represent whatever anatomical and physiological fact corresponds to the possibility of forming one connection or association or bond between an idea or any part or aspect or feature thereof and a sequent idea or movement or any part or aspect thereof . . . [Then] if we rank intellectual processes in a scale from lower, such as mere information, to higher, such as reasoning, the individuals who manifest the highest processes will have the largest number of *c*'s." Lashley (7, p. 173) says, "The capacity to form and to retain a variety of maze habits seems relatively constant for each individual dependent upon the absolute quantity of cortical tissue functional."

TABLE 1

ABSTRACTS OF ELEVEN INVESTIGATIONS (See footnote 2.)

Note: (1) Whenever probable errors and reliability coefficients were reported, these facts are noted under column "Method," though limited space makes it impossible to quote them all.

(2) For each investigation, the instruments are numbered *a, b, c,* etc., and these symbols are used in the column "Results."

Experimenter	Subjects	Instruments	Method	Results
1. May (9) (1921)	510 Army men	*Army Alpha* *a.* standard time *b.* double time	Group testing. Subjects worked for a period of standard time, then for another period of standard time using different colored pencils.	$r_{ab} = .965$
2. Ruch and Koerth (14) (1923)	122 college freshmen (Lowest and highest on Iowa Entrance Exam.)	*Army Alpha* *a.* standard time *b.* double time *c.* unlimited time	Group testing. Subjects worked for a period of standard time; then for another period of standard time; and then as long as they wanted, using different colored pencils for each period. Changes as well as additions allowed. *P.E.*'s reported.	$r_{ab} = .966$ $r_{ac} = .945$
3. Freeman, F. S. (4) (1931)	56 6th-grade pupils 51 8th-grade pupils 42 11th-grade pupils	*(6th) N. I. T.* *a.* standard time *b.* unlimited time *(8th) Otis Advanced* *c.* standard time *d.* unlimited time *(11th) Terman Group* *e.* standard time *f.* unlimited time	Group testing. First-day tests administered under standard time conditions. Next day same tests under unlimited time conditions. *P.E.*'s reported.	$r_{ab} = .83$ $r_{cd} = .58$ $r_{ef} = .93$

TABLE 1 (*Continued*)

Experimenter	Subjects	Instruments	Method	Results
4. Peak and Boring (12) (1926)	5 college students	*Army Alpha* *a.* speed (accuracy constant) *b.* unlimited time *Otis S. A.* *c.* speed (accuracy constant) *d.* unlimited time *x.* simple reaction-time to visual stimulus	Experimenter administered tests individually, timing each separate item with stop-watch. Speed score in terms of time on those items all subjects did correctly.	$\rho_{ax} = .70$ $\rho_{ab} = -.20$ $\rho_{cx} = .90$ $\rho_{cd} = .10$ $\rho_{xb} = 0$ $\rho_{xd} = 0$ $\rho_{bd} = 1.00$
5. Farnsworth, Seashore, and Tinker (3) (1927)	34 college students (divided into 5 groups for some computations)	*Army Alpha* *a.* speed (accuracy constant) *b.* standard time *Otis Advanced* *c.* speed (accuracy constant) *d.* standard time *e.* Ohio State Entrance Exam. *f.* Thorndike I. T. *x.* simple reaction-time *y.* serial discriminative reaction-time.	For speed scores, technique same as that of Peak and Boring. Ohio and Thorndike group tests under standard time conditions. Reliability coefficients for $x = .72$ for $y = .96$	ρ_{ax} and ρ_{cx} for 5 groups (N = 7) ranged from —.66 to .90 $r_{xy} = .15$ *b* *d* *e* *f* x —.22 —.24 —.16 —.16 y .55 .30 .14 .20

TABLE 1 (*Continued*)

Experimenter	Subjects	Instruments	Method	Results
6. Lemmon (8) (1927)	100 college students	*a.* Thorndike I. T. *b.* 3 learning tests *c.* 3 memory tests *x.* simple reaction-time to visual stimulus y_1 discriminative reaction-time y_2 " " y_3 " " y_4 " " y_5 " " Note: discriminations increased in difficulty from between 1 *vs.* no lights (y_1) up to 4 *vs.* 5 lights (y_5)	In discriminative reaction subjects released key corresponding to side of the board where greater number of lights appeared. Scores in memory and learning tests obtained from number items done in given amount of time (group testing). *P.E.*'s and reliability coefficients reported.	r_{xy_1} to r_{xy_5} show progressive decreases from .54 to .29. Intercorrelations of discriminative action-time: y_2 y_3 y_4 y_5 y_1 .58 .53 .42 .41 y_2 — .91 .85 .81 y_3 — — .92 .89 y_4 — — — .93 Thorndike I. T. with discrim. reaction-time: *r*'s increase with complexity of discrim. from .01 to .17 Learning with discrim. reaction-time: *r*'s tend to increase (up to .33) Memory with discrimination action-time: *r*'s tend to decrease (.17 to —.01)
7. Travis and Young (16) (1930)	111 college students (also results for 137 school children)	*a.* Reflex time in Achilles reflex *b.* Reflex time in patellar reflex *c.* Iowa Entrance Exam.	Group testing, Iowa Entrance Exam. *P.E.*'s and reliabilities reported.	$r_{ab} = .787$ $r_{ac} = .037 \pm .062$ $r_{bc} = .074 \pm .061$ (For group of children, results are similar.)
8. Rounds (13) (1928)	80 college students	*a.* addition *b.* letter crossout *c.* figure crossout *d.* completion *e.* word-object and word opposite *f.* Taylor recognition *x.* latent time in Achilles reflex	Same as that used by Peak and Boring for speed scores.	$r_{xa} = .44$ $r_{xb} = .49$ $r_{xc} = .51$ $r_{xd} = .44$ $r_{xe} = .21$ $r_{xf} = .28$

TABLE 1 (*Continued*)

Experimenter	Subjects	Instruments	Method	Results
9. McFarland (11) (1930)	34 college students	*a.* paper form board *b.* pencil maze *c.* insight (ingenuity) *d.* sentence completion *e.* opposites (simple) *f.* synonyms *g.* discrimination *h.* O'Connor number sense *i.* memory *x.* simple reaction-time (auditory stim.)	*P.E.*'s reported. Same as that used by Peak and Boring for speed scores.	*a b c d e f g h i* *x* .64 .62 .79 .71 .63 .58 .39 .11 .59 *a* — .85 .87 .72 .62 .50 .39 .00 .67 *b* — — .88 .77 .59 .48 .51 .25 .71 *c* — — — .83 .72 .62 .48 .26 .77 *d* — — — — .65 .56 .48 .25 .66 *e* — — — — — .76 .54 .31 .45 *f* — — — — — — .47 .23 .50 *g* — — — — — — — .44 .41 *h* — — — — — — — — .10
10. Dowd (2) (1926)	165 6th-grade children	*a.* 3 cancellation tests *b.* 2 tests of speed of hand movement c_1. speed writing sentences c_2. speed writing numbers *d.* speed of reading *e.* arithmetic *x.* Otis advanced	Group testing. Speed scores = amount done in given amount of time. Otis given under standard time conditions. Reliability coefficients on speed tests all above .90. Reliability for Otis (2 forms) = .83. *P.E.*'s reported.	Intercorrelations between speed tests range from $r_{de} = .151$ to $r_{c_1 c_2} = .810$. Intercorrelations of three *a*'s, all approx. .6, but lower with all other tests. Inter *r*'s between combined *a*'s, combined *b*'s, and combined *c*'s below .5. All speed measures with Otis = .1 or less, except $r_{dx} = .346$.
11. Clark (1) (1925)	180 7th- to 12th-grade children	*a.* Otis S. A. *b.* Terman Group *c.* Stanford Binet *x.* speed on arith. (lower levels I. E. R.) *y.* speed on completions (lower levels I. E. R.)	Same as that used by Peak and Boring. Standard procedure for tests *a*, *b*, and *c*. Reliability coefficients reported.	Correlations: *a b c* *x* .50 .60 .63 *y* .42 .28 .43 $r_{xy} = .42$ $r_{(x+y)\ (a+b+c)} = .56$

tellectual task than the individual with fewer connections (i.e., lower altitude) in whom the possible neural avenues are more limited in number. And the shorter the neural route, the greater the speed of response. Hence, any correlation between speed and altitude could be accounted for on the basis of number of available connections alone.

The above speculations may seem (though possibly intriguing) rather beside the point. However, since these two variables are difficult to isolate from each other, not only experimentally, but also in our concepts of them, devices such as these, which give a certain tangibility to our concepts, would seem justifiable.

Experimental Techniques and Results in Previous Studies

The experimental literature concerning this problem of speed and altitude has been reviewed rather fully several times [see McFarland (10) and Hunsicker (6)]. However, in none of these has there been a systematic attempt to examine techniques, instruments, and results in order to discover factors which have led to the conflicting conclusions of the investigators. Therefore it seems justifiable to examine a few typical investigations rather closely.

It is not surprising that we have a body of conflicting data and opinion as a result of the many studies conducted to investigate speed and altitude since (1) the variables to be measured are so difficult to isolate; (2) the instruments used to measure these variables have been almost as numerous as the investigations themselves; (3) frequently the evaluation of what the instruments measure has been uncritical; (4) the experimental procedures have varied widely; and (5) in many cases investigators have tended to bring to the problem biases which have carried them far beyond their data.

In Table 1 are presented abstracts of a few of the many investigations.[2] These investigations have been selected because they include some of the most quoted of the recent experiments, or because they include representative techniques and materials as well as diametrically opposed inferences or conclusions.

The first three investigations [May (9), Ruch and Koerth (14), and F. S. Freeman (4)] have in common virtually the same procedure, namely, standard group tests given under standard time conditions and then the same tests given under extended time conditions. The results are similar, namely, correlation coefficients are found of high or very high magnitude between scores under the two time conditions. May, and Ruch and Koerth were interested in determining for practical purposes the differential effect of different time allowances on test scores. Though these authors make no generalizations regarding the speed-altitude relationship, these two investigations have been interpreted by other writers as evidence of a pronouncedly high relationship [see (5, p. 97)].

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[2] Since space is limited, these abstracts attempt to give only that information which is pertinent to the present study.

F. S. Freeman, however, disagrees with those writers who regard such correlations as demonstrating a high community of function between speed and altitude. He says, "If under these conditions scores earned with a time limit imposed and scores on the same test without a time limit, there is a high correlation, then we may justifiably conclude that the test is primarily one of power." Freeman indicates that his results refute the assertions make by Peak and Boring (12) (the next study listed in Table 1).

Freeman believes further that the failure of his *r*'s to be unity indicates the existence of "a small but ever-present group of individuals who are penalized by an imposed time limit and whose test scores, therefore, are not truly representative of the relative levels they are capable of attaining." However, Freeman's correlations are not corrected for errors of measurement, and since unsystematic factors are probably operative, the evidence for his argument is inconclusive.

The fourth study listed in Table 1 is that by Peak and Boring, and the general conclusion drawn by these authors is that "speed of reaction is an important, and probably the most important, factor in individual differences in the intelligent act." It will be seen in Table 1 that the correlation coefficients are high between simple reaction-time and speed on Alpha and Otis test items with accuracy held constant (.70 and .90), but, on the other hand, the correlations between test scores in unlimited time and simple reaction-time, as well as between test scores in unlimited time and speed on the items with accuracy held constant, vary from —.2 to .1. The authors make no comment on the significance of these latter correlations and seem to rest their thesis on the high correlations between the several measures of speed. If correlational evidence derived from five subjects could be accepted, and if anyone were prejudiced in favor of the opposite theory (that speed of reaction is of little or no importance in individual differences in the intelligent act), these low correlations would support such a bias. The argument here must in a last analysis be reduced to a definition of "the intelligent act." Either Peak and Boring are calling the intelligent act the rate of accomplishing simple items which are within the comprehension of their subjects, or, if they include in the intelligent act the function we have termed altitude, the conclusions they draw from their data are contradicted by their own evidence.

However, since the probable errors of any computed constants with an N of five must necessarily be very large, it is possible that these obtained relationships are chance relationships. In fact, the evidence presented in the next study listed in Table 1, by Farnsworth, Seashore, and Tinker (3), indicates that they are chance relationships.

These investigators used the same technique as that of Peak and Boring, i.e., timing each separate item for each subject. They included the same tests plus certain other measures (serial action-time and two standard tests). Their group of subjects was larger ($N=34$), but when they attempted to hold accuracy constant on the several tests by using only those items which all subjects had done correctly, they found that there remained too few items for a reliable measure, so they split the group of 34 individuals up into five groups, each with an N of about seven. They then found that the correlations between simple reaction-time and time on the test items (accuracy constant) ranged from —.66 to .90 for the five groups (where Peak and Boring had obtained .70 and .90).

Between simple reaction-time and serial discriminative reaction-time these investigators obtained a correlation of .15 (using 34 subjects). The correlations between simple reaction-time and scores on four standard tests given under standard time conditions were all low and negative; between serial reaction-time and the four tests they were all positive but none of them high, ranging from .14 (Ohio State) to .53 (Army Alpha). These authors conclude, "The Army Alpha test tended to become, in the main, a serial action test (not a simple action test), but the Ohio State and Thorndike remained content tests in which the speed variable appeared to be negligible."

The next study appearing in Table 1 is that by Lemmon (8). There are many points in common between this and the preceding study—both simple and complex reaction-times were measured and at least one of the correlated measures, namely, the Thorndike Intelligence Test, was common to both. Lemmon's technique in measuring complex reaction-time was different from that in the previous study. He measured reaction-times on a series of discrimination problems of increasing difficulty (that is, from discrimination between one light *vs.* no light, up to four lights *vs.* five lights). Where Farnsworth, Seashore, and Tinker found a correlation of .15 between simple and serial reaction-times, Lemmon found a correlation of .29 between simple reaction-time and the most difficult discrimination reaction-time.

Lemmon's data indicate that as the discriminations became more difficult, the correlations between simple reaction-time and discriminative reaction-time became progressively lower (from .54 to .29). The same progressive tendency was manifest in the intercorrelations between the discriminative reaction-times, i.e., each discriminative reaction tended to correlate highest with those discriminative reactions closest to it in difficulty, and lowest with those most remote. Further, there was a marked progressive tendency for the absolute magnitude of the intercorrelations between the most difficult of the discriminations to be higher than between the easier ones.

Like Farnsworth, Seashore, and Tinker, Lemmon found that the Thorndike Intelligence Test tended to correlate higher with the more complex reaction-times than with the simpler (from .01 to .17). He found this same tendency with the learning tests, but just the opposite tendency with the memory tests, namely, a decrease in correlation with increased difficulty of discrimination. Lemmon says, "The correlations are all too low for purposes of prediction, but are sufficiently consistent in magnitude and sign to suggest relationships which are probably genuine." It is true that, though the differences are not statistically significant between correlations, the trends are fairly consistent. However, these correlations are so low as to be of little significance even for theoretical discussion.

In the next two studies [by Travis and Young (16) and by Rounds (13)] we find reflex times correlated with other measures. Travis and Young found a correlation between reflex time in the Achilles tendon reflex and scores on the Iowa Entrance Examination to be insignificantly different from zero (.037±.062). Rounds found that correlations between latent time in the Achilles tendon reflex and speed of accomplishing six mental tests ranged from .21 up to .51. It must be noted that while Travis and Young used as a correlated measure a test which was designed primarily to measure what we are calling altitude, the six mental measures used by Rounds were selected because they contained no difficulty range for his subjects (e.g., simple addition, crossouts, association tests, etc.), and if all subjects were allowed sufficient time, they could have all obtained perfect scores. Travis and Young conclude, "No evidence is found for correlation between reflex time and intelligence." Rounds concludes, "The latent time [in the Achilles tendon reflex] reveals a certain level of speed in that part of the nervous system which subserves the reflex. The correlation ratio points to a similar level of speed of transmission in other parts of the nervous system."

Rounds's statements are difficult to interpret. His data indicate that Pearsonian correlation coefficients were computed and not correlation ratios. His correlations are not corrected for attenuation, nor does he give reliability coefficients for the seven measures nor the intercorrelations between the six mental speed tests. The meagre information offered by the six raw correlations can give no inkling of the causal relations. Even though the true *r*'s between reflex latent time and the several mental speeds were all of the *same* magnitude, this fact would not indicate that each mental speed was "similarly" determined by latent time. For example, in one mental speed all of the factors entering into reflex latent time might be operative and in another mental speed only a few of them (assuming reflex latent time itself to be determined by complex causes). Many causal structures of the mental speeds by reflex latent time might be hypothesized, even though the true *r*'s between the mental speeds and reflex latent time were equal (and evidence from other investigations indicates that even this condition of equality of *r*'s would not hold, a fact which would further complicate the causal structures). For a presentation of the causal structures which might be put forth to interpret a given *r*, see Tryon (17, 18). Further, if the intercorrelations between speeds on these six mental tests varied [as intercorrelations between speeds on different tasks varied in the findings of McFarland (10) and Dowd (2)], say, all the way from .2 to .8, we would have to admit that other speed factors were operating in a differential manner for these variables, and hence that there are very dissimilar "levels of speed" either in the nervous system or in other mechanisms involved.

The next study listed is that by McFarland (11). Like Rounds, he arranged his experimental procedure in order to measure speed of accomplishing different tasks and likewise he had no measure of altitude. His study is divided into three parts, one with four subjects, one with five, and one with thirty-four. Only the last is summarized in Table 1. He used the same method as Peak and Boring, and others, that is, timing each individual item for each subject and holding accuracy constant by using only those items which all subjects did correctly.[3]

The intercorrelations between speeds of accomplishing the different tasks range from .00 to .88. The author concludes, "The results of these three researches agree with those of the previous investigators who have held that speed of reaction is one of the most important factors in individual differences in ability to react to mental tasks. It has been shown that subjects will maintain their relative speed rankings in various kinds of mental reactions." Unfortunately here again reliability coefficients are not presented and we have no way of knowing what the true relationships between these variables would be. However, the author seems unjustified in making the generalizations which he has from these data. Since the intercorrelations vary from .00 up to .88, with an average at about .54, the individuals obviously *do not* maintain their speed rankings; in fact, for purposes of prediction a majority of these correlations have little or no predictive value. McFarland also states, "The data obtained from these tests indicate that the same speed relationship exists between reactions of varying degrees of complexity." The above criticism would also hold for this statement. However, a further analysis could be made. A careful examination of McFarland's intercorrelation table indicates that certain of his measures, which, *a priori,* would seem to be measures of rather complex functions, namely, pencil maze, ingenuity problems, sentence completion, and paper form board, all show high intercorrelations with each other (from .722 to .880). Speeds on some of the other tests which *a priori* appear to measure relatively simple functions tend to show lower intercorrelations. Tests grouped in this way tend to corroborate results noted in Lemmon's study, that is, the intercorrelations between speeds on *difficult* discrimination reactions were of a much higher magnitude than those between speeds on the simpler discrimination reactions.

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[3] McFarland presents considerable argument for this method. But practical drawbacks of the method with regard to the experimenter's time are obvious. There is a further difficulty empirically demonstrated by the experiment of Farnsworth, Seashore, and Tinker, who found that even in a group as small as theirs (34 subjects) the number of items which all 34 subjects did correctly (on a standard test) was too small to give a reliable measure. McFarland says, "Certain factors, such as finger dexterity, fatigue, and practice effect tend to hide the actual relative speed rankings of subjects unless great care is taken in laboratory technique and procedure. These factors can be isolated and controlled only by timing each item of a test given to individuals rather than to groups of subjects." True, these factors and many others tend to affect relative speed rankings of subjects, both systematically and unsystematically. But McFarland does not indicate that he made any attempt to measure or control fatigue, practice effect, etc., in his experiment, nor *how* it could be done even by this testing method. Actually the method has tended to result in the use of small groups, thereby introducing large sampling errors, in some cases so large as to completely invalidate the results. Empirically, this method really arrives at a selection of material within the altitude range of all the subjects tested—a selection made by other workers on the basis of *a priori* judgments.

It is not this writer's wish to seem to argue that group testing method is more desirable, because advantages of the individual testing method are well known, but merely to point out that actually the individual method has resulted in increased errors of sampling or increased errors of measurement, or some compromise between the two [see Clark (1)].

But the most questionable region of McFarland's conclusions, from the present writer's point of view, lies in his assumption that his results indicate "a significant relationship between rate and mental ability." Here again, as with Peak and Boring's study, our argument must rest on a definition of terms. Since McFarland draws these conclusions from his data in which he so arranged his experimental set-up as to measure only rate or speed of accomplishing certain tasks (speed on tasks which all subjects finished correctly), he seems to be using the term "mental ability" synonymously with speed of accomplishing these tasks. It is the writer's feeling that the term "mental ability" in its general usage in psychological literature has a wider definition which includes altitude ability as we have defined it, and McFarland's study makes no attempt to measure this variable.

McFarland obtained higher correlations between simple reaction-time and speed on the various tests than did other investigators, with the exception of Peak and Boring (see Farnsworth, Seashore, and Tinker, and Lemmon). With two exceptions (discrimination and number sense), all of McFarland's correlations between simple reaction-time and speed on these tests ranged from .580 to .766. McFarland was using an auditory stimulus whereas in the other investigations a visual stimulus was used. Whether this factor accounts for the difference, it is impossible to say.

In the next study listed in Table 1 (by Dowd) the group method of testing was used throughout. Her method of holding "accuracy" constant in her speed tests was by selection of tests which, according to *a priori* judgment were well within the difficulty range of all the subjects. It will be noted that she used a large sample which was homogeneous in so far as grade in school is concerned. For all of her tests of speed she quotes reliability coefficients above .90, and for Otis Advanced, .83 (obtained by correlating alternate forms). She reports a range of intercorrelation between her different speed tests almost as great as that of McFarland (i.e., from .151 to .810). The lowest of these was between speed of reading and speed of doing arithmetic problems, and the highest between speed of writing sentences and speed of writing numbers. She interprets the difference between the low correlation coefficient she obtained between speed on arithmetic and speed on reading tests (.15) and the higher correlation (.50) obtained by Clark (See Table 1) between speed of sentence completions and arithmetic as a result of the difference in the instruments used. Clark's arithmetic problems were given in sentence form, e.g., "How many inches are there in two feet and seven inches?"; whereas her arithmetic problems were presented in some such form as $2 + 7 =$, or $3 \times 5 =$, etc. In other words speed of reading sentences was involved in both his arithmetic and his completion tests.

Dowd found both the combined scores on all speed tests as well as the scores on the separate tests (with the exception of the reading test) correlated .1 or less with scores on Otis advanced examination. The reading test correlated .346 with Otis. (It should be remembered that the Otis test has fairly severe time restrictions.) Dowd concludes, "It cannot be objected that the low correlations were due to low reliability, so it must be a fact that a person that is speedy in one thing has only a slight tendency to be speedy in another. High correlations are more likely to be due to similarity of material than to a general speed factor."

The next and last study listed in Table 1 is by Clark. He also used school children (180), but educational attainment in this group ranged from the 7th to the 12th grades. A spread in age and training as great as this might in itself account for a large part of the increased correlation which Dowd observed in certain of Clark's results.

Clark used the individual timing method for each item on his speed tests, but he, too, encountered the difficulty mentioned above (see footnote 3) of having insufficient items to insure statistical reliability when only those items were retained which all subjects had done correctly. Instead of subdividing his group as Farnsworth, Seashore, and Tinker did, he retained in his group all those individuals who had done at least 60% of the items correctly. Apparently the speed score then for each individual was the time in which he accomplished the items, whether correct or incorrect. But he ignores the fact that this finding might be interpreted to mean that *altitude ability was entering into the speed scores* to a certain extent due to individual differences in score on the remaining 40% of the items, and that the presence of these altitude factors in both kinds of tests might account for the correlation he obtained between speed and altitude. For example, the individual who got low scores on the altitude measures (Stanford-Binet, Terman, and Otis) might have found many of the items in the speed tests difficult and failed say up to 40% of them, thereby spending more time on them (according to an analysis made by Clark himself). This individual would have a poor speed score therefore as well as a poor altitude score. Further it should be pointed out that the three standard tests used to measure altitude were apparently given under standard time conditions, and hence speed factors were probably measured by them.

Summary and Interpretation of Theories and Results in These Selected Investigations

After this scrutiny of these selected investigations it becomes apparent that any generalized theories or laws regarding the nature of the speed-altitude relation are premature, especially since so few of the studies have taken account of sampling errors, errors of measurement, heterogeneity in irrelevant factors such as age and grade, obvious speed factors in the altitude scores and altitude factors in the speed scores. However, this collection of data seems to point to certain tendencies and implications which may be significant, namely,

1. With regard to speed:
    - *a.* Evidence seems to indicate that an individual who can accomplish one task rapidly may or *may not* show similar speed on

another task since correlations between speed of accomplishing different tasks vary all the way from zero to almost unity.

b. There appears to be a tendency toward higher correlations between speeds on those functions which are alike. [The reader is referred again to (2), (3), (8), and (10).] By "alike" we mean those functions which involve the same or some of the same psychobiological mechanisms. By this reasoning we would expect:

1) Speeds on very complex tasks to intercorrelate highly by virtue of the fact that the same or many of the same psychobiological mechanisms must necessarily be involved to a great degree, and

2) Less frequently would we expect two simple functions to correlate highly, and only when they involved fairly identical psychobiological mechanisms (see Dowd, speed of writing numbers and speed of writing sentences, or Travis and Young, speed of the Achilles tendon and the patellar tendon reflexes).

2. With regard to speed and altitude:

a. There seems to be little evidence for a high community of function between speed as measured by a variety of tests and altitude as measured by a variety of tests. In fact, the low order of correlation reported between speed and altitude in these investigations could for the most part be accounted for on the grounds that in some of the speed tests, altitude factors were being measured (e.g., Clark), and in nearly all the criteria of altitude (for the most part standard intelligence tests) we find speed factors not eliminated, that is, these tests were given under standard time conditions.

b. But aside from these ambiguous factors we may well inquire, would we expect a high correlation between speed and altitude unless we measure the speed and altitude of the same mechanisms, since correlations between speeds on what appear *a priori* to be fairly similar functions are so low? We still might find that speed is an important causal factor in altitude, or altitude an important causal factor in speed (see theories, page 198), if we could measure speed and altitude in the same function, since the experimental data reviewed here imply that there is a differential speed for different psychobiological mechanisms.

c. It might be argued that the studies of May, Ruch, and Koerth, and Freeman (since they used the same tests under both restricted and unlimited time conditions) succeeded in measuring speed and altitude in the same function. However, since altitude was allowed to vary in the speed test as well as in the altitude test, it could also be argued, as Freeman argues, that altitude ability alone was operating to cause the high correlation between scores under the two time conditions. His contention, however, does not shed any light on the theories cited earlier (pp. 198 ff.), namely, that both altitude and speed may depend upon some common factor or factors.

If speed and altitude are intimately associated in some such manner as that suggested by Lemmon, correlations obtained by such methods as Freeman's will not tell us what factors are determining the obtained correlation. And, since such a correlation will tell us nothing of the *causal relation,* new approaches will have to be made to this problem which will render new evidence. One such approach is suggested in a forthcoming paper by Tryon and Jones (19) together with results obtained by means of it.

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SUR LA NATURE DE LA "VITESSE" ET SA RELATION À D'AUTRES VARIABLES

(Résumé)

On nous donne ici une analyse comparative de onze études assez récentes et beaucoup cités sur la relation entre la vitesse de l'execution et une autre variable à laquelle on donne divers noms, tels que "altitude," "pourvoir," etc., et sur la relation, les unes aux autres, des vitesses de l'execution sur une variété de tâches. L'auteur cherche à déterminer la cause du conflict entre l'évidence et l'opinion à l'égard de ces relations en analysant (1) les instruments employés, (2) les processus expérimentaux, et (3) les partis pris qui ont influé sur l'évidence. L'auteur ne trouve aucune base pour une loi généralisée sur la nature de la relation entre la vitesse et l'altitude et la relation entre les vitesses, mais croit cependant que cet ensemble de données indique certains tendances. (1) La tendance d'un individu à montrer le même rang relatif en vitesse déprendra du degré auquel les deux tâches font employer les mêmes mécanismes psychobiologiques. (2) Les corrélations rapportées entre la vitesse et l'altitude dans ces investigations peuvent être expliquées pour la plupart par la raison que les variables de vitesse et d'altitude n'ont pas été complètement isolées l'une de l'autre dans la situation expérimentale, puisque les tests de vitesse fréquemment n'ont pas maintenu constants les facteurs d'altitude et on a administré les tests d'altitude dans des conditions de vitesse.

L'auteur suggère qu'une grande similarité de fonction entre la vitesse et l'altitude pourrait se montrer si l'on pouvait trouver des mesures de vitesse et d'altitude lesquelles isoleraient et mesureraient la vitesse et l'altitude dans les mêmes mécanismes psychobiologiques.

TRYON

ÜBER DIE BESCHAFFENHEIT DER "SCHNELLIGKEIT" UND IHRE BEZIEHUNG ZU ANDEREN VARIABLEN

(Referat)

Wir haben hier eine vergleichende Analyse von 11 ziemlich neuen und häufig zitierten Untersuchungen über die Beziehung zwischen Schnelligkeit der Ausführung (rate of performance) und einer anderen Variable die manchmal als "Höhe" (altitude), manchmal als "Kraft" (power), u.s.w. bezeichnet wird, und über die Beziehungen der Schnelligkeit der Ausführung verschiedenen Aufgaben unter einander. Die Verfasserin bersucht, den Grund der Widersprüche in den Beweisen und Meinungen über diese Beziehungen dadurch zu erforschen, dass er (1) die verwendeten Instrumente, (2) die experimentellen Verfahren, und (3) die Vorurteile, die die Evidenz beeinflussten, analysiert. Die Verfasserin findet keinen Grund für irgend ein allgemeines Gesetz in Bezug auf die Beschaffenheit der Beziehung zwischen "Schnelligkeit" und "Höhe" und zwischen den Schnelligeit en bei verschiedenen Aufgaben. Er glaubt aber, dass diese Datensammlung auf gewisse Tendenzen hinweist. (1) Der Grad bis zu dem ein Individuum dazu neigt [in verschiedenen Aufgaben], den selben relativen Rang zu erreichen in Bezug auf Schnelligkeit, wird auf den Grad beruhen, bis zu dem die zwei Aufgaben die selben Mechanismen in Anspruch nehmen. (2) Die gemeldeten Korrelationen zwischen Schnelligkeit und Höhe in diesen Untersuchungen kann grossenteils dadurch erklärt werden, dass die Variablen der Schnelligkeit und der Höhe in der experimentellen Situation nicht ganz von einander gesondert waren, da bei Schnelligkeitsprüfungen oft der Bestandteil der Höhe nicht konstant gehalten wurde, und bei Höheprüfungen die Schnelligkeit mit in Anspruch genommen wurde. Die Verfasserin weist darauf hin, dass eine starke Gemeinschaft der Funktionierung der Schnelligkeit und der Höhe sich vielleicht offenbaren würde, wenn Massstabe der Schnelligkeit und der Höhe erfunden werden könnten, die Schnelligkeit und Höhe in den selben psychobiologischen Mechanismen isolieren und messen würden.

TRYON

THE EFFECTS ON REACTION-TIME OF VARIOUS ORDERS AND DISTRIBUTIONS OF RELEVANT STIMULI IN THE WORD-ASSOCIATION EXPERIMENT*

From the Department of Psychology of the University of Oregon

B. K. Swartz and H. R. Crosland

I. Description of Technique, Etc.

A. Purpose

Several years of practical experience in measuring deception in subjects charged with stealing have convinced the senior author that the relative order of the *irrelevant* and *significant* word-stimuli in the word-association reaction-time experiment possesses some significance for the success or failure of the test. Consequently, an experiment was designed to measure what effects would result from five different orders or arrangements of the significant stimuli. A secondary and incidental purpose was also carried into effect, to measure the influence of practice or learning.

B. Word-Stimuli

Five sets of word-stimuli were manufactured. Each set contained 80 irrelevant and 20 relevant or significant nouns, the latter relating directly or indirectly to sex. Sets II, III, IV, and V contained as irrelevant stimuli words which were synonyms of the same type of words occurring in Set I. The significant stimuli were arranged to recur over and over again, in the remaining four sets or series, but in different orders and in different distributions throughout each set. These five sets of 100 word-stimuli we shall designate as Forms I, II, III, IV, and V. The irrelevant stimuli occurring in them may be seen by reference to Table 1. It has been deemed inexpedient to publish the list of 20 relevant stimuli. The positions occupied by them may be seen, however, by reference to Table 2 (page 221), which also presents symbols indicating their orders and their distributions.

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TABLE 1

Non-significant Stimuli

	Form I	Form II	Form III	Form IV	Form V
*1.	baseball	basketball	football	pastime	rugby
2.	Christmas	Thanksgiving	Easter	Monday	birthday
3.	vacation	freedom	holiday	spree	celebration
4.	grass	park	lawn	acre	field
5.	watch	timepiece	clock	an hour	minute
6.	history	geometry	chemistry	geology	economics
7.	telephone	phone	telegraph	microphone	radio
8.	vision	view	a sight	dream	a scene
9.	intelligence	sense	intellect	ability	brain
10.	train	locomotive	freight	express	streetcar
11.	sunshine	day	daybreak	morning	daylight
12.	music	tune	song	ballad	poem
13.	sickness	ill-health	illness	measles	health
14.	mountain	valley	hill	knoll	mound
15.	tree	shrub	bush	brambles	vine
16.	fun	joke	a riddle	story	a puzzle
17.	butterfly	insect	moth	a flea	spider
18.	fruits	meats	vegetables	tomato	carrots
19.	candy	chocolates	sweets	taffy	caramel
20.	whistle	horn	hum	squeak	yodel
21.	cold	cool	frigid	ice	a chill
22.	slowness	late	tardy	behind	early
23.	lake	pool	pond	water	puddle
24.	shoe	boot	slipper	heel	overshoe
25.	glove	sleeve	mitten	necktie	coat
26.	spider	fly	bug	worm	mosquito
27.	needle	thread	pin	thorn	spool
28.	tennis	handball	golf	bat	marbles
29.	movies	talkies	a show	a spectacle	a revue
30.	earth	map	globe	ball	sphere
31.	garden	yard	field	farm	section
32.	soldier	marine	sailor	doughboy	aviator
33.	cabbage	turnip	onion	raddish	bean
34.	eagle	bird	hawk	robin	sparrow
35.	sun	moon	star	planet	starlight
36.	bread	bun	biscuit	wafer	cake
37.	flag	banner	country	nation	emblem
38.	lamp	light	candle	lighter	a fire
39.	German	Spaniard	Frenchman	Russian	Swiss
40.	milk	tea	coffee	cream	chocolate

TABLE 1 (*continued*)

41.	cottage	home	house	a shed	a dwelling
42.	laugh	a sigh	tear	cry	a groan
43.	eat	dinner	a drink	sleep	food
44.	ocean	a sea	river	shore	lake
45.	sand	mud	gravel	stone	dirt
46.	stove	heater	furnace	radiator	fireplace
47.	cocacola	Green river	lemonade	gingerale	orangeade
48.	kodak	snapshot	camera	photograph	picture
49.	song	jazz	story	a tune	tale
50.	flowers	violets	roses	daisy	pansy
51.	hammer	a file	hatchet	a drill	axe
52.	thirst	famine	hunger	appetite	a desire
53.	city	town	village	New York	hamlet
54.	butter	egg	marmalade	cheese	jam
55.	teacher	professor	instructor	student	pupil
56.	joy	gladness	happiness	sorrow	sadness
57.	lion	wildcat	tiger	animal	cat
58.	pencil	eraser	pen	paper	ink
59.	moon	sun	planet	star	night
60.	scissors	clipper	shears	soap	pliers
61.	greenness	blue	pink	red	gray
62.	salt	cinnamon	pepper	seasoning	cloves
63.	a blossom	petal	bud	stem	leaf
64.	street	avenue	alley	sidewalk	boulevard
65.	artist	painter	sculptor	author	actor
66.	hat	derby	cap	lid	bonnet
67.	river	creek	stream	island	water
68.	a show	a revue	drama	a play	comedy
69.	nails	spikes	screws	hinges	a lock
70.	work	an effort	job	a boss	position
71.	a walk	tramp	a hike	a drive	picnic
72.	bones	muscle	skeleton	marrow	tendon
73.	book	pamphlet	magazine	biography	a novel
74.	a spring	summer	winter	autumn	a season
75.	a bridge	trestle	culvert	pathway	tunnel
76.	a play	amusement	a sport	pastime	player
77.	a glass	goblet	crystal	ruby	stone
78.	quietness	still	stillness	silence	noise
79.	auto	machine	car	buggy	carriage
80.	an orange	fruit	apple	grapefruit	lemon

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*These numbers, while arranged consecutively here, did not occur consecutively in the test itself. See Table 2 for the positions occupied by the *significant* stimuli.

Form I comprised 20 significant stimuli, arranged within the 80 non-significant stimuli, whose arrangement was such that it may be described as the *"center-bunch"* type of distribution. In the middle of the entire test, that is, after the first 40 irrelevant stimuli, all 20 significant stimuli are placed one after another, with no irrelevant stimulus interposed at any point between them; and then follow, from stimulus No. 61 on, the remaining 40 irrelevant stimuli without a break in this order. Form II may be characterized as the *"split-bunch"* type or form. One-half, that is 10, of the relevant stimuli occur exactly in the middle of the first half of the entire test (between stimuli 20 and 31) with no interrupting irrelevant stimuli anywhere between them; and the other half of them, the remaining 10, occur exactly in the middle of the second half of the entire test (between stimuli 70 and 81), coming one after another with no interspersed irrelevant stimuli to interrupt. Form III may be called the *"interspersed-regular"* form, since no two significant stimuli occur in immediate succession and since, however, every one of them occurs at fixed, regular intervals, that is, after every fourth irrelevant stimulus, beginning at stimulus No. 5 and continuing through No. 100. Form IV [adapted from Crosland's earlier work (21)] comprised three features: no two significant stimuli occur in immediate succession, the number of irrelevant stimuli between every two relevant words varies or is irregular, and approximately a half-half division of the 20 significant items occurs as between the upper and lower halves of the entire test (actually the division is represented by the ratio of 9:11). Hence, Form IV has been called the *"9:11 irregular."* And Form V is in every respect, save one, similar to Form IV: in it one-fourth of the significant stimuli, that is 5, occur in the first half, and three-fourths, the remaining 15, occur in the second half of the test. This form is called the "3:*2::6:9 irregular."* Reference to the various forms, Table 2, should suggest that the problem was to determine whether or not the differences in form would provoke differences in reaction-time (no consideration in th iswork is given, at present, to the various types of word responses of the subjects), to measure such differences if present, and to compare one form with every other form of the five. If anyone cares to accept one form as being the most efficient, he might argue that it should be the form whose reliability is satisfactorily high—the form whose irrelevant stimuli produce results most similar (as determined by inter-form correlation) to the results

TABLE 2

ORDER AND DISTRIBUTION OF SIGNIFICANT STIMULI IN FIVE FORMS OF THE WORD-ASSOCIATION TEST

Form I "Center-bunch"		Form II "Split-bunch"		Form III "Interspersed-regular"		Form IV "9:11 irregular"		Form V "3:2::6:9 irregular"	
Test word	Word position by no.	Test word	Word position by no.	Test word	Word position by no.	Test word	Word position by no.	word Test	Word position by no.
A	41	T	21	I	5	I	3	A	5
B	42	G	22	F	10	G	8	N	18
C	43	B	23	H	15	D	10	P	21
D	44	L	24	E	20	F	19	B	32
E	45	A	25	D	25	J	29	L	36
F	46	Q	26	B	30	O	34	E	51
G	47	C	27	Q	35	N	42	T	58
H	48	H	28	C	40	Q	45	K	60
I	49	K	29	A	45	H	49	M	64
J	50	D	30	G	50	M	54	R	71
K	51	O	71	S	55	L	61	S	74
L	52	M	72	K	60	R	63	C	76
M	53	N	73	R	65	P	69	H	82
N	54	J	74	P	70	B	76	D	85
O	55	P	75	N	75	E	79	O	87
P	56	I	76	T	80	A	84	J	91
Q	57	S	77	M	85	S	92	F	93
R	58	R	78	L	90	K	95	I	96
S	59	E	79	J	95	T	97	G	98
T	60	F	80	O	100	C	99	Q	100

from the irrelevant stimuli of every other form—the form whose relevant stimuli are in effect most like the relevant stimuli of every other form (this to be determined by correlation between them)—the form whose relevant items correlate *least* with its own irrelevant items—and the form which depicts the largest difference, in mean scores, between relevant and irrelevant items. Such a form, deemed most acceptable, should be one also to which certain facts in the literature can be significantly applied, that is, should be one which fits the logic which lies behind the association reaction-time technique. It is quite possible that no one form will outrank all other forms in *all* of these criteria.

Each form was mimeographed in duplicate. In the blanks of one copy reaction-time records were to be written; and in the blanks of the other, the subject's word-responses were to be recorded. Each sheet bore the subject's name, his experimental number, the form number, and the trial number. To prevent learning or fatigue from vitiating the records of any one form (or trial), a system of rotation was employed. Subject 1 had as Trial 1 Form I; and he concluded the trials with Form V. Subject 2 had as Trial 1 Form II; and he concluded the five trials with Form I. Thus every form occupied every position as to trial number in an equal number of presentations.

C. Subjects

Forty-nine men students at the University of Oregon, ranging in age from 19 to 29 years, served in the experiment. They were chosen at random (on a basis solely of accessibility) without regard to university class or fraternity affiliation. More than one-half were freshmen, and they were equally divided as between fraternity affiliation and its absence. Every subject submitted voluntarily, and, indeed, many of them sought of themselves to take part in the test.

D. Procedure

Two assistants aided the experimenter in giving each test. All three were males. The chief experimenter, following the system as developed by Crosland (21), assumed the function of pronouncing the word-stimuli and of starting and stopping the stop-watch. Assistant No. 1, sitting at the former's right, recorded time records, reset the watch, and placed the unused (second) watch in a position accessible to the right hand of the experimenter. It is to be gathered that two watches were employed in alternate fashion, thus to secure speed in the presentation of the stimuli and to promote regularity in their presentation. Assistant No. 2 sat at the experimenter's left and assumed the function of writing down, in prepared blanks here as in the case of the time records, the subject's various word-responses. Neither assistant was to interrupt during the presentation of the 100 stimuli unless forced to do so by inability to understand the subject's reply or the failure of the watch to function, etc. A third stop-watch was kept in reserve in case of an accident to either of the other two. To prevent distracting noises, the watches were placed upon and moved on a cloth pad. The watch was started at the last syllable of the experimenter's pronunciation and was stopped by him at the first sound of reply emitted by the subject.

Not more than three subjects were tested at one period (usually in the evening). Each subject was submitted to the entire five trials (forms) at one session, a 3-minute rest period being allowed between each two forms. The entire test, for each subject, consumed approximately 45 minutes.

As we have said above, all subjects served voluntarily. The procedure of approach consisted of the experimenter or an assistant interviewing a subject, after the evening meal, in the lounging room of the dormitory or other living-quarters. It was signified that in a nearby room the association-test was being administered and that his participation would be greatly appreciated. If he assented to being tested, he was escorted to the experimental room, seated as per the prearranged conditions, given the customary instructions, and submitted to the progress of the test to its completion.

The subject was requested to be seated in a chair located approximately 2 feet from the edge of the table on the opposite side of which sat the experimenter and his two assistants. This table was of standard height and was 30 inches wide. Its length was such as comfortably to accommodate, on only one side, the three testers with all of their record blanks, watches, etc. Two desk lights were arranged to furnish light to the testers and also to reveal clearly the facial expressions of the subject.

Properly seated, the subject listened to the reading of typed instructions, which were as follows:

> "This test is being given for the personal use of the experimenter, and your cooperation will be greatly appreciated. Your name will not appear in the finished work. This is distinctly not a test for detecting guilt, so you have nothing to fear from it.

TABLE 3

ODD-EVEN COEFFICIENTS OF RELIABILITY FOR EACH FORM OF TEST, IRRESPECTIVE OF TRIAL, FOR NON-SIGNIFICANT AND SIGNIFICANT REACTION-TIMES, PRESENTED AS RAW AND AS STEPPED-UP r'S ($r_{1.I}$) (DOUBLING LENGTH OF TEST)

(N=49)

Nature of stimuli	Form of test					Basis of correlation	Trial I
	I	II	III	IV	V		
Non-sigs. (raw)	.91±.02	.85±.03	.94±.01	.93±.01	.93±.01	(40:40)	.96±.03 raw
$r_{1.I}$ (stepped-up)	.95	.92	.97	.96	.96	(80:80)	.98 $r_{1.I}$
Sigs. (raw)	.73±.05	.66±.05	.84±.04	.73±.05	.60±.06	(10:10)	.52±.07 raw
$r_{1.I}$ (stepped-up)	.84	.80	.86	.84	.75	(20:20)	.68 $r_{1.I}$

> You are asked to respond as quickly as possible to each of the words which he will pronounce to you. Please be sure to respond with the first single word which occurs to you; do not respond with a sentence or phrase."

The instructions were followed by an illustration, such as: stimulus, "black-board"; response, "chalk." The subject was then asked if he understood the conditions and what he was to do. When he answered in the affirmative, the signal, "Ready!" was given, and the experiment began with the pronunciation of the first word-stimulus. It continued as rapidly as the replies of the subject and the speed of the recording of the word-responses by assistant No. 2 would permit. The subject had very little, if any, time for collecting himself or for planning evasions, thanks to the system of alternate stop-watches and the employment of two assistants, leaving the chief experimenter free to pronounce and to start and stop the watches.

Upon the completion of all five tests, the subject was requested not to discuss the experiment with his companions, so that they, yet to be tested, would not be affected by such prior information. He was thanked and escorted from the room.

II. RESULTS

A. Reliabilities

The coefficients of reliability in this experiment have been computed in two ways: (1) by obtaining mean scores (for each subject) of odd items *versus* even items, in each of the non-significant and the significant reaction-times, and the running of correlations in each

TABLE 4

INTER-ALTERNATE-TRIAL COEFFICIENTS OF RELIABILITY ($r_{1.I}$), IRRESPECTIVE OF FORM OF TEST, FOR NON-SIGNIFICANT AND SIGNIFICANT REACTION-TIMES ($N=49$)

Nature of stimuli	Trial No.	Trial number			Mean $r_{1.I}$ (non-sig.)	Mean $r_{1.I}$ (sig.)
		1	2	3		
Non-sigs.	3	.812±.034			.8445	.706
Sigs.	3	.783±.037				
Non-sigs.	4		.825±.032			
Sigs.	4		.720±.046			
Non-sigs.	5	.848±.028		.893±.022		
Sigs.	5	.546±.068		.775±.040		

TABLE 5

RANK ORDERS OF FORMS OF TEST BY MAGNITUDE OF RELIABILITY $(r_{1.\mathrm{I}})$

(Taken from Table 3)

Significant stimuli Form	 r	Non-significant stimuli Form	 r
III	.86	III	.97
I and IV	.84	IV and V	.96
II	.80	I	.95
V	.75	II	.92

form between these odd and even mean scores; and (2) by correlating one trial with every other, alternate trial (irrespective of form) in each of the significant and the non-significant mean time scores. These coefficients of reliability are shown in Tables 3 and 4, respectively, for the odd-even and the inter-trial comparisons.

With reference to the above (Table 3) coefficients of reliability, it should be recalled that the number of subjects was 49, and that our experiment, involving correlation as it does at so many points, deals with individual differences. Hence, if we follow Kelley,[1] the raw reliabilities of the *non-significant* reaction-times are ample, for they approach closely 0.9, and the population exceeds 30. However, the raw reliabilities of the *significant* items fail to measure up to this standard. Hence, since odd-even comparisons divide the test in half, thus reducing its true length, we are justified in transmuting the raw r's $(r_{\frac{1}{2}.\frac{1}{2}})$ (especially in the latter case) into what they would be were the odd and even items of the divided test as numerous as in the true test itself $(r_{1.\mathrm{I}})$. Having thus applied the Brown-Spearman formula, we obtain as the lowest reliability of significant stimuli a coefficient of .75, that for Form V. The highest reliability obtains for Form III, a value of .86. These values are still under .9. The foregoing two statements do not include reference to Trial 1, in which all Forms were amalgamated, since our quest relates to efficiency of form and not to efficiency of any one trial. It is true, however, that Trial 1 possesses for its significant items a stepped-up $(r_{1.\mathrm{I}})$ reliability of only .68. Similarly, considering the reliabilities for the non-significant reaction-times, we find the lowest to be .92 (Form II) and the highest to be .97 (Form III). Arranging a rank-order table for each class of stimulus, from greatest reliability $(r_{1.\mathrm{I}})$ to least, we obtain Table 5.

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[1] See Hunter (47*a*).

As a whole, without regard to form of test, our experiment is proven (by values contained in Table 4) to have possessed reliabilities of questionable adequacy with respect to the relevant items. For the significant stimuli, these values range from .546 to .783, with a mean r of .706; while the reliabilities of the non-significant stimuli approach adequacy by ranging from .812 to .893, with a mean r of .845. Cognizance will be given later in this paper to the reduced nature of these reliability coefficients: other r's derived from the tests will be corrected for attenuation, since in this manner a coefficient of correlation obtained between two tests of low reliability can be augmented in size and its significance shown to be greater.

B. Relationships Existing between the Five Forms of the Test

1. *By Coefficients of Correlation*

a. Relationship between the non-significant reaction-times. The actual raw scores of each subject may be observed in Table 6. These are mean scores, because each subject was in each form of the test submitted to 80 irrelevant or non-significant stimuli, and to 20 significant stimuli. Hence, the 80 items give him a *non-sig.* mean and the 20 items a *sig.* mean score. Table 6 also shows the group ($N=49$) mean score, the group distribution σ, and the σ of the group mean score, for each of the *non-sig.* and *sig.* stimuli in each form of the test.

The mean (individual) scores in Table 6 become useful for purposes of comparing the five forms. We shall first compare them with respect to reaction-time to the non-significant stimuli. This can be done, first, by running correlations between the individual mean scores for this type of stimulus as between the five forms. The coefficients of such correlations appear as Item A of Table 7.

In Table 7 it is clear that we have a small range of inter-form correlations for the irrelevant stimuli, from .72 to .87, a range of only .15. Hence, all forms of the test, so far as the non-significant stimuli are concerned, are very similar. This is as it should be, for one of the fundamental hypotheses behind the word-association reaction-time experiment is that the test should reveal the presence of emotion or emotional conflict through its significant stimuli, principally, and through their contrasts with the irrelevant stimuli. If one strikes a mean of the inter-form r's, just referred to, one will find that the forms arrange themselves in the following rank order: V, .815; I, .802; II, .792; and III and IV, .770. Hence, if one cares to reason

TABLE 6

MEAN SCORES OF INDIVIDUAL SUBJECTS, FOR EACH TYPE OF STIMULI, IN EACH OF THE FIVE FORMS OF TEST; THE GROUP ($N=49$) MEAN SCORES, THE σ'S OF DISTRIBUTION, AND σ'S OF THE GROUP MEANS
(These values represent seconds.)

	Form I		Form II		Form III		Form IV		Form V	
Subj.	N-sig.	Sig.	N-sig.	Sig.	N-sig.	Sig.	N-sig.	Sig.	N-sig.	Sig.
1	2.7	3.7	2.9	3.5	2.4	3.7	2.3	2.3	2.4	2.7
2	2.5	2.1	2.0	3.0	2.2	3.0	2.3	2.1	2.1	1.9
3	1.8	2.0	1.6	1.6	1.8	2.6	2.0	2.3	2.0	2.1
4	1.6	2.0	1.7	1.8	1.7	1.6	2.0	3.3	1.8	2.0
5	2.0	2.3	1.8	2.0	1.8	1.5	1.6	1.4	2.1	2.7
6	2.7	4.0	2.3	2.8	2.0	2.0	1.8	1.8	1.9	1.8
7	1.5	1.7	1.8	2.8	1.6	1.9	1.6	1.9	1.5	1.5
8	1.4	1.7	1.7	2.1	1.8	3.5	1.8	1.9	1.8	1.7
9	1.4	1.3	1.2	1.4	1.3	1.3	2.0	2.8	1.6	3.3
10	1.4	1.7	1.4	1.4	1.3	1.3	1.3	1.3	1.4	2.2
11	1.7	2.6	1.6	2.2	1.7	1.8	1.6	1.8	1.5	1.7
12	1.6	1.6	1.8	2.8	1.6	2.1	1.5	1.9	1.6	1.8
13	1.7	1.8	1.7	1.6	2.1	4.2	1.8	2.0	1.7	1.9
14	1.6	1.5	1.6	1.5	1.5	1.6	2.1	2.2	2.0	2.1
15	1.8	2.3	1.8	1.9	1.7	2.0	2.0	1.9	2.2	2.3
16	2.2	3.4	2.2	3.2	1.0	3.3	1.0	2.1	1.9	1.8
17	1.5	1.4	1.7	2.6	1.6	1.6	1.6	1.3	1.6	1.8
18	1.5	1.5	1.4	1.2	1.6	2.0	1.6	1.6	1.6	1.8
19	1.6	1.6	1.5	1.4	1.4	1.4	2.0	2.6	1.7	1.9
20	2.3	3.1	2.5	3.2	2.6	3.0	2.4	2.9	2.4	3.3
21	1.4	2.0	2.0	1.3	1.3	1.2	1.2	1.1	1.2	1.0
22	1.2	1.2	1.3	2.0	1.8	1.2	1.3	1.0	1.1	1.3
23	1.2	1.4	1.2	1.2	1.5	1.9	1.4	1.3	1.3	1.3
24	1.6	1.7	1.6	2.0	1.5	1.5	1.7	1.7	1.6	1.4
25	1.5	1.2	1.3	1.3	1.1	1.2	1.1	1.1	1.5	2.1
26	1.2	1.5	1.3	1.7	1.1	1.3	1.1	1.1	1.2	1.0
27	1.5	1.4	1.8	3.0	1.7	2.0	1.5	1.5	1.7	1.6
28	1.1	0.9	1.2	1.0	1.5	1.3	1.3	1.4	1.1	1.0
29	2.2	1.2	1.7	2.5	1.5	1.9	1.6	1.5	1.5	1.7
30	1.6	1.8	1.6	1.4	1.4	1.2	1.5	1.4	1.7	2.3
31	1.4	1.8	1.3	1.2	1.2	1.3	1.2	1.1	1.1	1.0
32	1.8	1.7	1.7	1.6	1.9	1.9	1.9	1.8	1.7	1.9
33	1.2	1.2	1.2	1.0	1.2	1.1	1.4	1.6	1.3	1.5
34	1.6	1.8	1.5	1.7	1.5	1.6	1.9	1.8	1.8	2.1
35	2.2	3.1	2.4	2.4	2.2	2.7	2.5	2.2	2.2	2.1
36	1.6	1.8	1.7	1.7	1.3	2.0	1.8	1.7	1.7	1.9
37	1.6	1.5	1.4	1.3	1.8	2.1	1.7	1.7	1.5	1.6
38	2.2	2.0	1.9	1.6	1.7	1.5	2.1	2.3	2.2	2.7
39	2.0	2.7	1.7	2.0	1.6	2.0	1.7	1.7	1.9	4.1
40	1.3	1.5	1.3	1.5	1.3	1.6	1.3	1.3	1.3	1.4
41	1.2	1.3	1.3	1.5	1.2	1.1	1.3	1.4	1.2	1.0
42	2.5	3.5	2.6	2.5	2.9	3.5	2.6	4.1	2.4	3.8
43	1.8	1.7	1.6	1.6	1.8	1.5	2.2	2.7	1.9	1.6
44	1.4	2.3	1.5	1.8	1.4	1.5	1.4	1.5	1.4	2.4
45	1.8	3.3	2.0	2.0	1.7	1.5	1.8	1.7	1.8	1.4
46	1.7	2.2	1.8	2.4	1.8	2.4	1.7	1.7	1.6	2.0
47	1.1	1.1	1.2	1.2	1.4	1.3	1.4	1.3	1.2	1.1
48	2.5	2.1	2.5	2.0	2.3	1.7	2.6	3.7	2.4	2.1
49	1.7	2.1	1.7	1.8	1.5	1.6	1.6	1.7	1.8	3.0
Mean total	1.76	1.96	1.76	1.97	1.72	1.92	1.79	1.83	1.75	1.94
σ dist.	.42	.729	.398	.638	.375	.738	.378	.645	.362	.677
σ mean	.06	.104	.057	.091	.053	.105	.054	.092	.052	.097

TABLE 7

Item A

Coefficients of Inter-Form Correlation as between Reaction-Time Scores for Non-significant Stimuli
(N=49)

Forms	I	II	III	IV
II	.86±.03			
III	.77±.04	.80±.03		
IV	.74±.04	.72±.05	.75±.04	
V	.84±.03	.79±.04	.76±.04	.87±.02

Item B

Coefficients of Inter-Form Correlation as between Reaction-Time Scores for Significant Stimuli
(N=49)

Forms	I	II	III	IV
II	.59±.06			
III	.50±.07	.59±.06		
IV	.41±.08	.65±.05	.41±.08	
V	.48±.07	.30±.09	.30±.09	.53±.07

Item C

Inter-Stimulus Relationship (Coefficient of Correlation) as between Reaction-Time Scores in Each *Form* of the Test
(N=49)

Forms	I	II	III	IV	V
Sigs. *vs.* Non-sigs.	.77±.04	.73±.05	.75±.04	.86±.03	.66±.06

from such small differences, and if one assumes that the best test will be one which is most like other tests of reaction-time to irrelevant stimuli, then one may declare Form V the best test and Forms III and IV the poorest.

b. Relationship between the significant reaction-times. However, preliminary assumptions, and the results described in the literature of the subject, force our attention to the effects in reaction-time to the *significant* stimuli of the various forms of the test. Item B, Table 7, contains the inter-form *r*'s for the significant reaction-times. Here the range is more considerable, from .30 to .65, amounting to as much as .35, this range being twice as large as that which obtains for the non-significant, inter-form correlations. Ranking our forms again, on the basis of size of mean inter-form *r*'s, we discover that the forms arrange themselves in the following order, from the greatest unlikeness to other forms to the least: V, .402; III, .450; I, .495; IV, .500; and II, .532. Hence, reasoning as above, we must regard Form V as being the least efficient form of the five employed in this investigation and Form II the most efficient, followed closely by Form IV.

c. Relationship between the significant and non-significant time scores in each form alone. A really efficient word-association reaction-time test should cause striking differences to appear as between the mean significant-stimuli reaction-times and the mean irrelevant-stimuli reaction-times. Hence, it is assumed that the form of test which exhibits the most striking difference between the two should be, all else being equal, the most efficient form to employ. Let us look into this question by observing Item C, Table 7. One finds there the coefficients of correlation between the *significant* and *non-significant* reaction-time records, in each form of the test. The inter-relationships depicted range from .66 to .86, a range amounting to .20. Ranking the forms, again, from greatest difference (between *sig.* and *non-sig.* time) to least, we discover that the forms arrange themselves as follows: V, .66; II, .73; III, .75; I, .77; and IV, .86. Hence, we may say (as assumed above) that Form V is the most, and Form IV the least, efficient test.

The reader may, perchance, object and say that any raw coefficient of correlation is of doubtful value, because it alone does not indicate the factors which are common to the two variables—that it would be wise to evaluate more properly the significance of the r's between the *sig.* and *non-sig.* time records in Forms I through V. This may be done in several ways. We shall correct them for attenuation, for this method considers the reliability of each variable entering into a given r; and then, by the most conservative method known, namely, by squaring each raw r, the percentages of common factors will be computed; the corrected r's will also be squared; and then we shall compute K, the index of alienation, to obtain the percentage of uncommon factors as between the five forms. The results of these computations may be observed in Table 8.

Regardless of the point of view from which we may consider the degree of commonality, as between the *sigs.* and the *non-sigs.,* it is clearly evident that Form V possesses the smallest number of factors common to the two variables; hence, it is the best *of these five* forms of test. This paper makes no attempt to say to what degree the percentage of commonality could be further reduced, thus to increase the

TABLE 8

PERCENTAGES OF COMMON FACTORS BETWEEN THE VARIABLES KNOWN AS REACTION-TIMES TO SIGNIFICANT AND NON-SIGNIFICANT STIMULI IN EACH OF FIVE FORMS OF THE WORD-ASSOCIATION REACTION-TIME TEST

Form	Raw r	r corrected for atten.	atten. r^2	raw r^2	$K^2 =$ 1—raw r^2	K	1.00—K
I	.77	.86	.74	.59	.41	.64	.36
II	.73	.85	.72	.53	.47	.69	.31
III	.75	.82	.67	.56	.44	.66	.34
IV	.86	.96	.92	.71	.29	.54	.46
V	.66	.79	.62	.44	.56	.75	.25

effectiveness of Form V. Comparisons of raw r's, comparisons of r's corrected for attenuation, comparisons of squared raw r's, comparisons of squared corrected r's, and comparisons of the values of (1.00—K) are all of a piece in showing Form V to possess the smallest percentage of factors as common to the two kinds of reaction-time. Should we rank the forms under each method of computation, we should discover Form V in the leading position and Form IV in the most adverse position throughout, with but small differences existing between the remaining forms. Form V is nearer to Forms II, III, and I, in smallness of common factors, than Form IV is similar to any other form. In other words, Form IV is, on this basis, most decidedly the poorest form of the lot.

Lest the differences between the correlation coefficients, Tables 7 and 8, as between each form and every other form, be challenged by the statement that such differences between the inter-form r's are lacking in significance, it will be of advantage to compute the significance of each in terms of probability. Such differences, with the probability expression of the significance of each, appear in Table 9.

It should be quite clear from Item A, Table 9, that many of the differences between inter-form r's for the significant stimuli are significantly large. The mean difference is itself the largest for those comparisons of r's where one or both members of the comparison involve Form V. Likewise, Form IV comes next in size of mean difference between r's, followed in order by Form II, and Forms I and III. Similarly, the degrees of probability (significance) are greatest for all comparisons involving Form V, followed in order by Forms II, IV, and I and III. It is obvious in the table that Form V, involved in

TABLE 9

DIFFERENCES BETWEEN THE INTER-FORM AND INTER-STIMULUS COEFFICIENTS OF CORRELATION AND THE SIGNIFICANCE OF EACH IN TERMS OF PROBABILITY

A. Between Inter-Form r's—for Reaction-Times to Significant Stimuli

Forms involved		$r_{1,2}$ = .59	$r_{1,3}$ = .50	$r_{1,4}$ = .41	$r_{1,5}$ = .48	$r_{2,3}$ = .59	$r_{2,4}$ = .65	$r_{2,5}$ = .30	$r_{3,4}$ = .41	$r_{3,5}$ = .30	$r_{4,5}$ = .53
$r_{1,3}$;	Diff.	.09									
	Prob.	73									
$r_{1,4}$;	Diff.	.18	.09								
	Prob.	87	71								
$r_{1,5}$;	Diff.	.11	.02	.07							
	Prob.	77	55	66							
$r_{2,3}$;	Diff.	.00	.09	.18	.11						
	Prob.	50	73	87	77						
$r_{2,4}$;	Diff.	.06	.15	.24	.17	.06					
	Prob.	68	86	94	88	68					
$r_{2,5}$;	Diff.	.29	.20	.11	.18	.29	.35				
	Prob.	100	96	82	94	100	100				
$r_{3,4}$;	Diff.	.18	.09	.00	.07	.18	.24	.11			
	Prob.	87	71	50	66	87	94	82			
$r_{3,5}$;	Diff.	.29	.20	.11	.18	.29	.35	.00	.11		
	Prob.	100	96	82	94	100	100	50	82		
$r_{4,5}$;	Diff.	.06	.03	.12	.05	.06	.12	.23	.12	.23	
	Prob.	65	58	77	63	66	82	99	77	99	

Comparisons containing	Mean difference	Mean probability
V's	.165	82
IV's	.135	79.2
II's	.126	79.5
I's & III's	.113	75

(Table 9 (*continued*)

B. Between the Inter-Stimulus *r*'s (*Sig. vs. Non- Sig.* Data) in the Various Forms of the Test

	I (.77)		II (.73)		III (.75)		IV (.86)	
	Sig. *vs.* Non-sig.		Sig. *vs.* Non-sig.		Sig. *vs.* Non-sig.		Sig. *vs.* Non-sig.	
Form	Diff.	Prob.	Diff.	Prob.	Diff.	Prob.	Diff.	Prob.
II (.73)	.04	67						
III (.75)	.02	59	.02	58				
IV (.86)	.09	90	.13	94	.11	93		
V (.66)	.11	84	.07	74	.09	79	.20	96

Comparisons containing	Mean difference	Mean probability
V's	.12	83
IV's	.13	93
III's	.06	72
I's	.065	75
II's	.065	73

an r compared with some other r, gives several probabilities as high as 100 chances in 100.

In somewhat similar fashion, Item B, Table 9, dealing as it does with r's between *sig.* and *non-sig.* reaction-times, reveals that Forms V and IV are almost tied in the amount of mean difference between the r's in comparisons in which they are involved; also that the size of difference for them is much higher than for the three other forms. In degree of probability, Form IV leads with a mean of 93, followed by V with 83, I with 75, II with 73, and III with 72.

In other words, Forms IV and V are far more dissimilar to the other forms than they are dissimilar to one another. And the size of this dissimilarity is almost large enough to remove chance factors from consideration. The dissimilarity is shown both in the reaction-time r comparisons for the significant stimuli alone (A) and in the differences between r's which involve the excess reaction-time of the relevant stimuli as contrasted with the irrelevant stimuli (B). These two forms, of the five, are the most outstanding forms of the test.

2. *By Differences between the Means of the Five Forms and the Significances of These Differences.* There is still another method which may be employed to evaluate the efficiency of the various forms of the word-association test. The method of computing the differences between the mean scores and of determining the significance of these differences will now engage us. Table 10 presents the differences between form-mean scores ($N=49$ subjects) and the significance of each expressed in terms of probable chances in 100 that retests would reveal a similar difference. The data of Table 10 are subdivided into Items A, B, and C, to correspond to the differences between form-mean scores for each of the irrelevant and the relevant time records and in each form between the relevant and the irrelevant data, respectively.

To comprehend thoroughly the facts revealed in Items A and B of Table 10 it will not be amiss to arrange the forms of the test in rank orders, by sizes of differences and by the degrees of significance of these differences. To make such a rank-order table (Table 11) complete, we shall incude in it the ranks on absolute mean time scores also (taken from Table 6).

From the method depicted in Table 11 it is clearly evident that Form I is in the lead in the positive direction (direction of utility, if the reasoning adoptted be correct), while Form IV is way out in

TABLE 10

DIFFERENCES BETWEEN FORM MEANS OF REACTION-TIMES (A) TO NON-SIGNIFICANT STIMULI AND (B) TO SIGNIFICANT STIMULI, AND BETWEEN THE NON-SIGNIFICANT AND SIGNIFICANT MEANS IN EACH FORM (C); THE STANDARD ERRORS OF THE DIFFERENCES; AND THE PROBABILITIES THAT SUCH DIFFERENCES ARE REAL DIFFERENCES

A. Reaction-Times to Non-significant Stimuli

Forms→	I				II				III				IV			
Forms	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability
II	.002	.030	.066	52												
III	.038	.040	.950	83	.040	.035	1.140	86								
IV	.039	.041	.950	83	.037	.041	.900	82	.077	.039	1.970	97				
V	.008	.033	.240	60	.010	.035	.286	61	.030	.036	.830	79	.047	.027	1.740	96

B. Reaction-Times to Significant Stimuli

Forms→	I				II				III				IV			
Forms	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability	Diff.	$\sigma_{diff.}$	$\frac{\text{Diff.}}{\sigma_{diff.}}$	Probability
II	.007	.089	.079	52												
III	.040	.104	.384	64	.047	.090	.520	69								
IV	.136	.108	1.260	89	.143	.110	1.300	90	.096	.108	.888	80				
V	.018	.102	.176	57	.025	.110	.230	59	.022	.119	.184	57	.118	.092	1.28	89

C. Each Form's Difference between Non-Sig. and Sig. Mean Reaction-Times

Forms→ I				II				III				IV				V			
D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.
.206	.069	2.98	99.8	.211	.062	3.40	100.0	.204	.074	2.75	99.7	.031	.054	.574	71.0	.196	.073	2.68	99.6

TABLE 11

Rank Orders of Forms of the Test in Absolute Mean Time Scores, Sizes of Differences between These Means, and Magnitude of Significance of differences (Items A and B of Table 10)

(— to + or + to — refers to order of greater utility to less)*

Rank order	Absolute form means (Table 6) Non-sigs. (— to +)	Absolute form means (Table 6) Sigs. (+ to —)	Table 10's diffs. between form means Non-sigs. (— to +)	Table 10's diffs. between form means Sigs. (— to +)	Degrees of probability for the differences in columns 3 & 4 Non-sigs. (— to +)	Degrees of probability for the differences in columns 3 & 4 Sigs. (— to +)	Mean rank order
1	III	II	I & II	I	I & II	I & V	I
2	V	I	V	III	V	II & III	II
3	II & I	V	III	II	III	—	V
4	—	III	—	V	—	—	III
5	IV	IV	IV	IV	IV	IV	IV

*The reasoning involved here assumes that the best word-association test will cause the shortest (most nearly normal reaction-times to the irrelevant stimuli and the most greatly lengthened times to the relevant words—that the smallest difference between form means commends the form in question, and that the greatest chance differences, as between forms, commends the form in question.

the lead in the other direction. It is surprising and disappointing that Form V did not lead in producing the biggest difference between the absolute mean *sig.* time and the absolute mean *non-sig.* time (Item C, Table 10) ; but it is gratifying that the significances of the differences between Form V and all other forms for the mean time to the *sigs.* (Table 10, Item B) fell below those for Form IV, thus suggesting greater chance differences and hence a greater similarity of Form V to the other forms. The reason for the former fact undoubtedly arises from the high degree of correlation which obtains between reaction-times for the non-significant and the significant stimuli. This correlation, however, is least for Form V (see Item C, Table 7), and may be regarded as one virtue of the latter form.

In view of the seeming disparity between what is revealed as differences between forms of the tests by the inter-form coefficients of correlation and the differences between form means, it seems wise to point out here that the method of obtaining a correlation coefficient possesses two inherent characteristics which make it the more useful method for differentiating between forms. In the first place, any correlation is based fundamentally upon the amounts of variation of all scores each taken from the mean of all scores for each variable. Hence, it is immaterial what absolute value the mean possesses. And, since this statement applies to each variable entering into the correlation, the two means could still be equal (with a difference equal to zero) and low correlation obtain, and the means could vary widely from each other and still a high correlation could obtain. And, in the second place, no matter how a correlation computation is considered, if fundamentally depicts the commonality or degree of association existing between the *rank orders* into which the two variables have thrown the various subjects submitted to them. Yet the rank order into which subjects are thrown is a most effective way of regarding the individual differential effects of the variable upon all subjects. Hence, an inter-form correlation is the most stringent method of bringing to light differences between forms of the test inasmuch as such differences bring out differences between the various subjects. On this basis, if our reasoning is sound, we may ignore, in this connection and for the purpose of evaluating the differences between the forms of the test, the small differences (and the reduced significance of each) as between form-mean reaction-time scores. If our purpose were merely one of determining which form elicits the largest reaction-time to significant stimuli, this contention would not be cogent. Such, however, has not been the purpose of the investigation.

What differences in group-mean reaction-time scores accrued from the form of the test, and in each form as between the significant and non-significant stimuli, may easily be observed in Figure 1, which has been constructed from the data of Table 6. One will note that Form

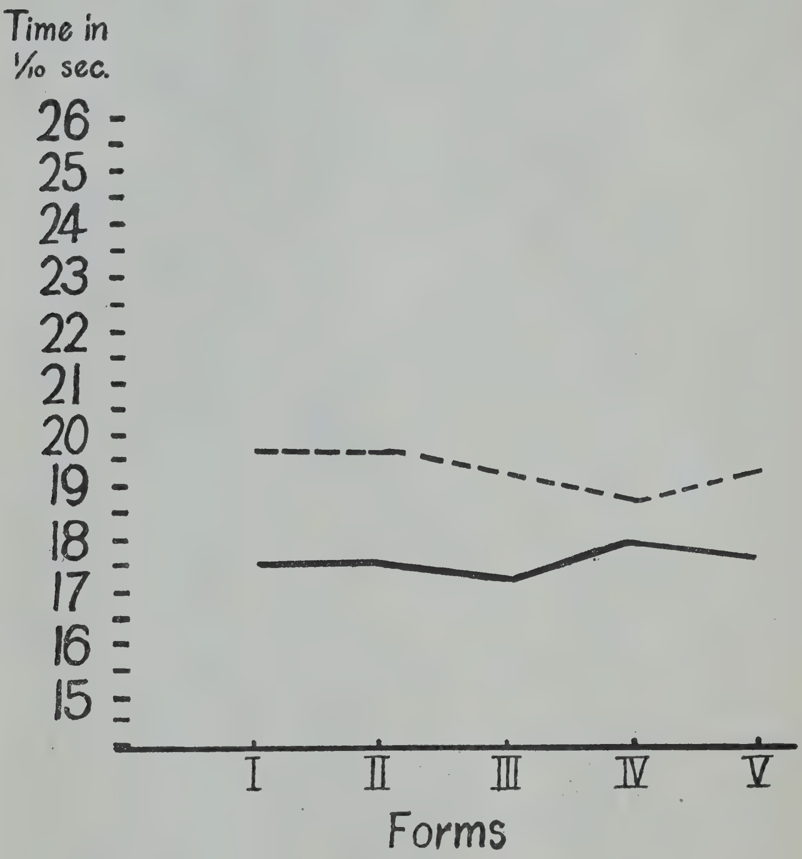

FIGURE 1

Comparison of Mean Reaction-Times to Significant and Non-significant Stimuli for Each of the Five Forms

(Constructed from Table 6)

Sigs. — — — —

Non.-sigs.————

IV gives the smallest absolute difference between the *sigs.* and the *non-sigs.* Except for Form IV the forms are much alike. Form V perhaps is nearest to Form IV in the smallness of this difference. Certain it is that Forms I, II, and III are very similar. Considering merely the mean times to significant stimuli, Forms I and II are tied with highest reaction-time means, followed in order by Forms III and V, with Form IV giving the lowest reaction-time mean. Similarly, with respect to only the means to the non-significant stimuli: Form III gives the lowest value, followed very closely by Forms I and II, then V, with Form IV giving the highest value. On account of the fact that the presence of post-significant items, considered without distinction from the non-significant data, may have had some influence upon these absolute values and the relative differences between them, one should not accept these comparisons, from Table 6 and Figure 1, uncritically. In Section IV one will find a table (Table 16) and a discussion appertaining to the effects of the post-critical words when included among the irrelevant items.

Item C, Table 10, deals with the significance of the difference between the non-significant and the significant stimuli mean times in each form of test. From it two points are clear: There is little to choose as between Tests I, II, III, and V, since the probability in each is as high as 99.6 chances in 100. And Test IV exhibits a difference between *sig.* and *non-sig.* time whose significance is as low as to be represented by only 71 chances in 100.

Inasmuch as the difference, in mean time, between the *sigs.* and *non-sigs.* in Form IV was so low, with a corresponding reduction in the significance of this difference, two further methods of evaluating the forms with respect to differences between the means of the *sigs.* and the *non-sigs.* suggest themselves. First, let us investigate the number of *plus* and *minus* differences in each form by individual subjects. And, secondly, we may run correlations of such differences as between forms. Taking up the former at once, we may consult Supplementary Table A.

Earlier in this paper it was necessary to express some surprise at the smallness of the difference between the *sig.* and *non-sig.* mean time in Form IV. Supplementary Table A depicts an explanation of this low magnitude of difference and also offers its own causes for conjecture. It is disappointing that many of the mean reaction-times to irrelevant stimuli, as between individual subjects, were larger than those to the relevant stimuli, with many of them being *minus* instead

SUPPLEMENTARY TABLE A

Individual Differences between *Sig.* and *Non-sig.* Time in Each Form

			Forms		
Subject	I	II	III	IV	V
1	+1.0	+0.6	+1.3	+1.0	+0.3
2	—0.4	+1.0	+0.7	—0.2	—0.2
3	+0.2	0.0	+0.8	+0.3	+0.1
4	+0.4	+0.1	—0.1	+1.3	+0.2
5	+0.3	+0.2	—0.3	—0.2	+0.6
6	+1.3	+0.5	0.0	0.0	—0.1
7	+0.2	+1.0	+0.3	+0.3	0.0
8	+0.3	+0.4	+1.7	+0.1	—0.1
9	—0.1	+0.2	0.0	+0.8	+1.7
10	+0.3	0.0	0.0	0.0	+0.8
11	+0.9	+0.6	+0.1	+0.2	+0.2
12	0.0	+1.0	+0.5	+0.4	+0.2
13	+0.1	—0.1	+2.1	+0.2	+0.2
14	—0.1	—0.1	+0.1	+0.1	+0.1
15	+0.5	+0.1	+0.3	—0.1	+0.1
16	+1.2	+1.0	+1.4	+0.2	—0.1
17	—0.1	+0.9	0.0	—0.3	+0.2
18	0.0	—0.2	+0.4	0.0	+0.2
19	0.0	—0.1	0.0	+0.6	+0.2
20	+0.8	+0.7	+0.4	+0.5	+0.9
21	+0.6	—0.7	—0.1	—0.1	—0.2
22	0.0	+0.7	—0.6	+0.6	+0.2
23	+0.2	0.0	+0.4	—0.1	0.0
24	+0.1	+0.4	0.0	0.0	—0.2
25	—0.3	0.0	+0.1	0.0	+0.6
26	+0.3	+0.4	+0.2	0.0	—0.2
27	—0.1	+1.2	+0.3	0.0	—0.1
28	—0.2	—0.2	—0.2	+0.1	—0.1
29	—1.0	+0.8	+0.4	—0.1	+0.2
30	+0.2	—0.2	—0.2	—0.1	+0.6
31	+0.4	—0.1	+0.1	—0.1	—0.1
32	—0.1	—0.1	0.0	—0.1	+0.2
33	0.0	—0.2	—0.1	+0.2	+0.2
34	+0.2	+0.2	+0.1	—0.1	+0.3
35	+0.9	0.0	+0.5	—0.3	—0.1
36	+0.2	0.0	+0.7	—0.1	+0.2
37	—0.1	—0.1	+0.3	0.0	+0.1
38	—0.2	—0.3	—0.2	+0.2	+0.5
39	+0.7	+0.3	+0.4	0.0	+2.2
40	+0.2	+0.2	+0.3	0.0	+0.1
41	+0.1	+0.2	—0.1	+0.1	—0.2
42	+1.0	—0.1	+0.6	+1.5	+1.4
43	—0.1	0.0	—0.3	+0.5	—0.3
44	+0.9	+0.3	+0.1	+0.1	+1.0
45	+1.5	0.0	—0.2	—0.1	—0.4
46	+0.5	+0.6	+0.6	0.0	+0.4
47	0.0	0.0	—0.1	—0.1	—0.1
48	—0.4	—0.5	—0.6	+1.1	—0.3
49	+0.4	+0.1	+0.1	+0.1	+1.2
Mean of all diffs.=	+0.27	+0.22	+0.25	+0.17	+0.28
Total No. +'s	30	26	29	23	30
Total No. —'s	13	12	12	15	14

SUPPLEMENTARY TABLE B

Coefficients of Inter-Form Correlation between the Differences between Reaction-Times to Relevant and Irrelevant Stimuli

Forms	I	II	III	IV
II	+0.059			
III	+0.277	+0.316		
IV	+0.120	—0.053	+0.018	
V	+0.119	—0.040	+0.051	+0.201

with *P.E.*'s ranging from ±.0953 to ±.0856

of *plus* differences. The most numerous of these were in Forms IV and V, contrary to expectations, although the differences in their frequency were not great as between the five forms. It is possible to account for the increase in reaction-time to the *non-sigs.* in these two forms but that the increase should, in so many cases, be so large as to exceed the *sig.* times is difficult of explanation. The most accessible explanation is that each of these series, by its system of irregular distribution of the relevant stimuli, induced great cautiousness on the part of the various subjects when dealing with even the irrelevant stimuli. Another is that, in Form V, too few *non-sigs.* were interspersed among the *sigs.* which had been placed in greatest frequency in the last 50 words of the test list. Again, it is possible that the relations between the subjects and the experimenter, and the social situation of the test, were such as not to induce greatest embarrassment or severe conflict of emotion when critical stimuli were presented.

Adoption of the second procedure referred to above has led to correlations as between forms with respect to the differences contained in Table A. The *r*'s so obtained comprise Supplementary Table B.

It is to be noted that every *r* in Supplementary Table B is very low; in fact, four of them are smaller than their *P.E.*'s, and the remaining five, while larger than the *P.E.*'s, are still not large enough to arouse comment. If we group them, in spite of their low values, by form of test, and get a mean *r* for each form, we obtain the following rank order of such mean *r*'s: III (+0.1655), I (+0.144), V (+0.083), IV (+0.0715), and II (+0.0705). Because of the difficulty of interpreting the meaning of an *r* between differences, we are perplexed to evaluate the above rank order. The assumption that the form giving the highest mean *r* is the best form certainly does not place either Form IV or Form V in this favorable position. Conversely, the assumption that the lowest mean *r* indicates the preferable form places neither Form IV nor Form V in the favored position. However, two aspects are to be noted: Forms IV and V do rank side by side, with almost identical mean *r*'s; and Forms I and III are very much alike in the size of mean *r*'s. If *r*'s smaller than their *P.E.*'s are ignored, and if the mean *r*'s obtained from the remaining five coefficients are reranked, the following rank order results: II, III, I, IV, and V. This ranking would induce us to adopt the converse assumption above, namely, that the smallness of the inter-difference (between *sig.* time and *non-sig.* time) coefficients of correlation is symptomatic of greater efficiency of the form of test. At any rate, Forms IV and V are similar. It should likewise be noted that it was these forms which showed the only negative *r*'s of Table B, a fact which accords with the assumption just stated, especially since the negative *r*'s contrast each of Forms IV and V with Form II. It is quite possible that the factors invoked on a former page to explain the small differences as between the *sig.* and the *non-sig.* mean times have not only affected the sizes of the differences but also the inter-form-differences coefficients of correlation, lowering the latter and making them less useful for the purpose of throwing further light upon the efficiency of Forms IV and V. With the data at hand the quest cannot go farther.

C. The Effects of Repetition of the Test

1. *By Inter-Trial Coefficients of Correlation*

a. Relationship between (a) non-sig. time records; (b) sig. time records; and (c) the sigs. and the non-sigs. in each trial. Whether the foregoing differences, and the significance of each (Table 10), really arose from the forms of the test or from chance factors, we may attempt to demonstrate by employing the very same (individual-mean) scores but grouped according to trial. Obtaining a group mean for each trial, and subtracting one group mean from another, the size of the difference becomes apparent, and its significance is then computed. Such differences *between* trials and *in* each trial, with the significance of each, comprise Table 12.

The trial differences, and their significances, as shown in Table 12, would seem to suggest seven conclusions: (1) The system of rotating the forms, as between trials, especially when the differences between forms were smaller than the differences between trials, served

TABLE 12

DIFFERENCES BETWEEN TRIAL MEANS OF REACTION-TIMES (A) TO IRRELEVANT STIMULI AND (B) TO RELEVANT STIMULI, AND (C) BETWEEN THE RELEVANT AND IRRELEVANT MEANS IN EACH TRIAL; THE STANDARD ERRORS OF THE DIFFERENCES; AND THE PROBABILITIES THAT SUCH DIFFERENCES ARE REAL DIFFERENCES

A. Reaction-Times to Irrelevant (*Non-sig.*) Stimuli

Trials	Trials→ I				II				III				IV			
	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.
II	.07	.073	.96	83												
III	.15	.069	2.17	98	.08	.076	1.05	85								
IV	.23	.062	3.71	100	.16	.119	2.32	98.9	.08	.065	1.23	88				
V	.23	.069	3.33	100	.16	.076	2.11	98	.08	.072	1.11	86	.00	.000	.00	50

B. Reaction-Times to Relevant (*Sig.*) Stimuli

Trials	Trials→ I				II				III				IV			
	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.
II	.50	.101	4.95	100												
III	.75	.098	7.65	100	.25	.130	1.92	97								
IV	.88	.083	10.60	100	.38	.115	3.30	100	.13	.110	1.18	87				
V	.97	.048	20.20	100	.47	.093	5.05	100	.22	.090	2.44	99.2	.09	.074	1.21	88

C. Each Trial's Difference between *Non-Sig.* and *Sig.* Mean Reaction-Times

Trials→ I				II				III				IV				V			
D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.	D	σ_d	$\frac{D}{\sigma_d}$	Prob.
.70	.123	5.68	100	.27	.108	2.51	99.4	.10	.101	.99	84	.05	.088	.568	71	.04	.075	.54	69

to counterbalance form against form in such a manner that the differences as shown must have resulted principally from the trials, hence they must in large part be due to the effects of practice or learning. (2) Non-contiguous trials show the biggest differences, one from another, and the most significant differences. (3) Contiguous trials, before the limits of learning have been approached, show the next smaller differences, with slightly reduced significance. (4) And contiguous trials, after the limits of learning have been reached, show the smallest differences and lowest significance. (5) The differences between trials in the reaction-times to the relevant stimuli are larger than those for the irrelevant stimuli, with a corresponding difference in degrees of significance. (6) The most pronounced learning or adaptation occurred with respect to the significant stimuli; hence several inter-trial differences are larger, the limits of learning were reached sooner, Trial II showed the biggest increment of difference per trial, and all probabilities were increased over those present in the irrelevant stimuli records. (7) The differences between the *sig.* and *non-sig.* mean time scores, in each trial, reflect tendencies similar to those indicated above in conclusions 2, 3, and 4. We may more clearly envisage these summarized results if we will group trials together and thus present a rank-order table based on the size of each difference and the degree of its significance, as shown in Table 13.

One additional point concerning practice effects, taken from Table 12, is worthy of particular note. As one progresses from left to right over Item C (Table 12), from Trial I to the next trial, and so on, one will observe that the probabilities decrease. And since this item represents the difference, in each trial, between the *sig.* and *non-sig.* means, one must conclude that learning, or practice, or adaptation reduces in increasing amounts, with repetition, the significance of each difference. Or to state the matter conversely: the most significant differences can be discovered between the significant and non-significant stimuli, as they affect reaction-time, when learning (or its equivalent) has not yet occurred. And after it has occurred the difference becomes smaller and smaller and the number of chance factors reducing the size of the significance of any difference increases. It should be pointed out, however, that as far as this investigation went (through five trials) the reduction in probability resulting from practice did not proceed beyond a point represented by 73 chances in 100. Yet, it appears that more chance factors operated in producing the differences between forms than between the trials, and that the differences

TABLE 13

Comparative Differences between Trials, and between Groups of Trials, with Respective Degrees of the Significance of These Differences

By Rank

Order	*Irrelevant* times Trials	Diff.	Prob.	*Relevant* times Trials	Diff.	Prob.
1	I *vs.* IV	.230	100	I *vs.* V	.970	100
	I *vs.* V	.230	100	——	——	——
2	(I *vs.* IV)+(II *vs.* V)	.195	99	I *vs.* IV	.880	100
3	II *vs.* IV	.160	98.9	I *vs.* III	.750	100
	II *vs.* V	.160	98	——	——	——
4	(II *vs.* IV)+(I *vs.* III)	.155	98.5	(I *vs.* IV)+(II *vs.* V)	.675	100
5	I *vs.* III	.150	98.4	(I *vs.* III)+(II *vs.* IV)	.565	100
6	——	——	——	I *vs.* II	.500	100
7	(II *vs.* IV)+(III vs. V)	.120	92.45	(I *vs.* III)+(III *vs.* V)	.485	99.6
8	(I *vs.* III)+(III *vs.* V)	.115	92	II *vs.* V	.470	100
9	——	——	——	II *vs.* IV	.380	100
10	III *vs.* V	.080	86	(II *vs.* III)+(I *vs.* II)	.375	98.5
	II *vs.* III	.080	85	——	——	——
	III *vs.* IV	.080	88	——	——	——
11	(I *vs.* II)+(II *vs.* III)	.075	84	(II *vs.* IV)+(III *vs.* V)	.300	99.6
12	I *vs.* II	.070	83	II *vs.* III	.250	97
13	——	——	——	III *vs.* V	.220	99.2
14	(III *vs.* IV)+(IV *vs.* V)	.040	69	III *vs.* IV	.130	87
15	——	——	——	(III *vs.* IV)+(IV *vs.* V)	.110	87.5
16	IV *vs.* V	.000	50	IV *vs.* V	.090	88

as between trials are larger than the differences resulting from the various forms of the test. Consequently, the attempt to balance off practice effects, one against another, in the results grouped according to form, by the rotation system of presenting the forms to the subjects, did not prove to be wholly successful. Thus practice effects may represent one set of the chance factors which operated in reducing the significance of the form differences in Table 10.

2. *By Mean-Trial Differences as between Forms.* There remains the question of whether or not practice affected equally the five *forms* of the test, and, if its effects were unequal, what form was least affected by it? This question we may now consider by regarding the contents of Table 14, in which are arranged the mean (group) reaction-time scores according to *trial* under each *form*.

From this table (Table 14) it appears that Form II, in *irrelevant* reaction-times, resisted learning or other effects of repetition most greatly, followed in order by Forms V, III, I, and IV. The differences, in mean gain from the last four trials, range from .8 to 1 per cent (Form II) to 4 per cent for Form IV. Hence, the gain in Form IV is 5 times that of Form II; and in Form V it is 2.5 times the gain in Form II. But for the reaction-times to the relevant stimuli the situation is somewhat different. While here Form IV resisted rather than encouraged learning most greatly, being tied in this respect by Form III, the difference between the least learning (Forms IV, III) and the greatest (Form I) is so small that we must decide that there is little to choose as between the forms for the significant stimuli; they are, with respect to significant stimuli, almost equally affected by learning. Expressing, however, such differences as exist as multiples of the smallest gain, we may say that Forms V and II showed each a gain which is 1.11 times the gain in the latter forms. In general, the conclusion suggests itself that perhaps Form IV, possessing greatest susceptibility to gain for one type of stimulus and least for the other, is the most different from all other forms of the test; and Form V, ranking low in susceptibility to gain for one type of stimulus and not the highest in susceptibility for the other type, is very much like other forms; hence, on this type of criterion, Form V cannot be said to be more virtueless than the other forms; it is not the form least lacking in the property of resisting practice effects. If the results, on gain, for the two types of stimuli are thrown together, Form II is least resistant to practice effects, followed in order closely by Forms V and III, then Form IV, and

TABLE 14

Percentage Gains, from Trial to Trial, in Each Form of the Test

(by Group Mean Scores)
(N=10)

	Non-significant Stimuli						*Significant* Stimuli				
Form→ Trial	I	II	III	IV	V	Form→ Trial	I	II	III	IV	V
1	1.86	1.69	1.83	1.97	1.82	1	2.69	2.42	2.41	2.44	2.65
2	1.73	1.93	1.64	1.75	1.80	2	2.13	2.18	1.92	1.94	2.03
Diff. in %	— .07	+ .14	— .10	— .11	— .01	Diff. in %	— .21	— .10	— .20	— .20	— .23
3	1.77	1.68	1.68	1.62	1.63	3	1.67	1.85	1.68	1.71	1.82
Diff. in %	+ .02	— .10	+ .02	— .07	— .09	Diff. in %	— .22	— .15	— .13	— .12	— .10
4	1.57	1.65	1.59	1.67	1.57	4	1.71	1.68	1.69	1.65	1.64
Diff. in %	— .11	— .02	— .05	+ .03	— .04	Diff. in %	+ .02	— .09	+ .01	— .04	— .10
5	1.59	1.57	1.59	1.65	1.65	5	1.58	1.53	1.60	1.66	1.59
Diff. in %	+ .01	— .05	0	— .01	+ .05	Diff. in %	— .08	— .09	— .05	+ .01	— .03
Mean Diff.	— .038	— .008	— .032	— .040	— .022	Mean Diff.	— .12	— .11	— .09	— .09	— .11

Mean diff. in % of *all* stimuli

I	II	III	IV	V
—.080	—.060	—.061	—.065	—.066

lastly by Form I. It must be confessed that this method of comparing the forms has proved disappointing, although the biggest difference accrues to Form I and on the basis of this fact alone we should be disposed to regard Form I as being an undesirable method of employing the word-association reaction-time test. On this basis the other forms appear to be much alike one another.

3. *By Group-Mean Differences as between Trials* (*Irrespective of Form*). It is wise to consider the effects of learning in still another fashion. In the preceding section they have been considered as accruing from the form of the test. We shall here ignore form and throw all ($N=49$) Trial I scores into group-mean scores, and so on

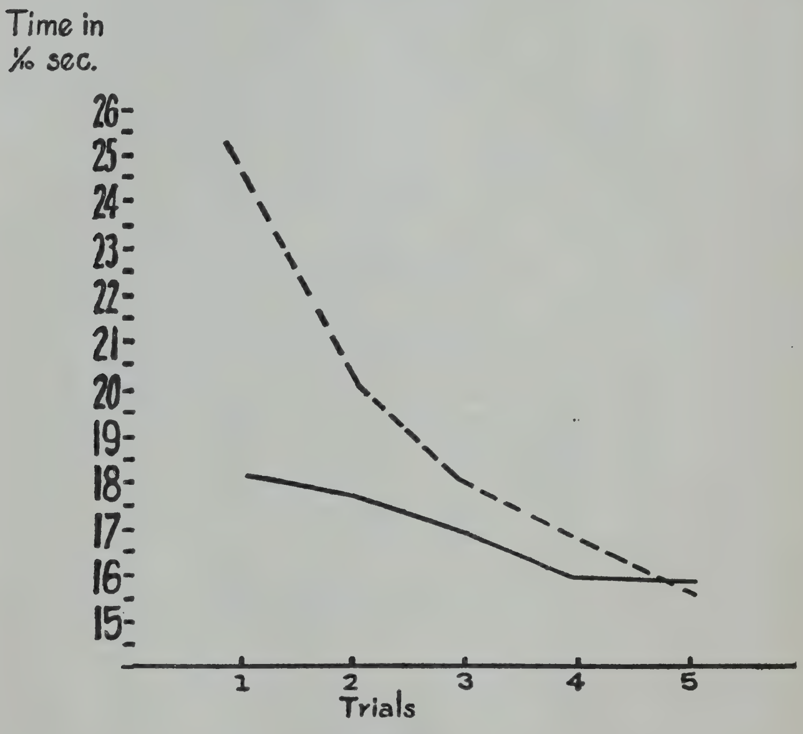

FIGURE 2

EFFECT OF PRACTICE ON TIME OF RESPONSE

(Constructed from Table 15)

Sigs. — — — —

Non.-sigs.————

TABLE 15

Group-Mean Time Scores to Significant and Irrelevant Stimuli Arranged According to Trial (Ignoring Form) and the Group-Distribution σ's of Variation

Trials→	I		II		III		IV		V	
	Mean	σ	Mean	σ	Mean	σ	Mean	σ	Mean	σ
Sigs.	2.54	.77	2.04	.64	1.79	.60	1.66	.49	1.57	.40
Non-sigs.	1.84	.39	1.77	.40	1.69	.36	1.61	.38	1.61	.34

for each of Trials II through V. These group-mean scores by trials may be seen in Table 15. Their graphic representation comprises Figure 2, constructed from the contents of Table 15.

It is evident from Table 15 and Figure 2 that some, but slight, learning occurred in the reaction-times to the irrelevant stimuli. What practice effects did occur characterized Trials III and IV, as against II and I. This result is not surprising when it is recalled that the irrelevant stimuli, from form to form, were different, though synonymous, and the trials rotated the forms, thus cancelling the forms against one another. It is, likewise, very evident that great amounts of learning occurred in reaction-times to the relevant stimuli, which had been arranged to recur in different orders throughout all trials. The total amount of this learning, from Trial I to Trial V, amounted to as much as 38% of the first trial score, whereas for irrelevant stimuli this reduction through practice amounted to only 12.5%. The curve for the significant reaction-time means is a beautiful example of the typical learning curve, since the most precipitous drop lies at Trial II, the next at Trial III, and the least at Trials IV and V. Such practice effects are so great for the reaction-times to the significant stimuli that in Trial V they are even shorter and faster than those to the irrelevant stimuli. What would have been the effect of practice had a woman experimenter tested all male subjects or had the social situation furnished more urgent motives for evasion or conflict cannot be stated, but it seems reasonable to suppose that the tests, as executed, did not put a premium upon, or greatly provoke, embarrassment or fear or a conflict between two or more emotions. On the other hand, practically every investigation in the past, where repetitions have been employed, has indicated learning or practice effects in reaction-times to word-association stimuli. And it is noteworthy in this investigation that one set of 20 relevant stimuli functioned throughout all five forms and all five trials.

D. Influences Exerted by Perseveration Effects on the Time Scores for Irrelevant Stimuli

All tables, computations, comparisons, deductions, etc., up to this point, involving or being based upon irrelevant stimuli time records, have made no distinction between true non-significant time data and data derived from stimuli which followed immediately after the critical words in each form of the test. This may prove to be the poorest feature of the whole experiment, for should a strong perseverative effect carry over from each "indicator" to the word just following, then the mean time for the irrelevant stimuli becomes unduly weighted with the slow reactions resulting from the perseverations. This fact would result in only a small difference occurring between the mean significant time and the mean non-significant time. Let us re-classify the data to discover the size of the post-significant mean in each form of the test, and then we may reconsider some of the inter-form correlations which have heretofore been presented. Table 16 compares the mean times for significant and post-significant stimuli.

A glance at Table 16 should cause a rejection of Forms I and II from consideration, since each individual mean (comprised in the group mean) for post-significant stimuli was determined from too few cases. This leaves merely Forms III, IV, and V to be compared. It is clear that in Form IV the post-significant reaction-times were approximately equal to the significant times, hence the non-significant records in Form IV are shown to have been heavily weighted or increased by the inclusion in them of the post-significant items. This charge does not apply with such great force to Forms III and V. Form IV, it is recalled, presented an exceedingly high correlation (.86) between its significant and non-significant reaction-times. Undoubtedly, the inclusion of the *post-sigs.*, as *non-sigs.*, helped to raise the correlation to this value. If such contamination occurred in Forms III and V, it occurred to a much lesser degree. But, if one insists that ratios of .93 and .92 do not differ too greatly from .98, then it is proper to insist that the *r*'s between *sigs.* and *non-sigs.* in Forms III and V were also raised, that the real inter-stimulus correlation values were less than the obtained values of .75 and .66, respectively. This deduction does not in any way cast reflections upon the efficiency of Form V, in so far as it gave the lowest correlation between *sigs.* and *non-sigs.* Certain former conclusions are here reinforced so that we may now begin to recognize at least one disadvantage under which Form IV has been compared with all other forms: for reasons not

TABLE 16

GROUP-MEAN TIME SCORES FOR THE SIGNIFICANT AND THE POST-SIGNIFICANT STIMULI COMPARED (N=49)

Forms→	M	I N=stimuli basis each subject's score	M	II N=stimuli basis each subject's score	M	III N=stimuli basis each subject's score	M	IV N=stimuli basis each subject's score	M	V N=stimuli basis each subject's score
Sigs.	1.96	20	1.97	20	1.92	20	1.83	20	1.94	20
Post-sigs.	1.57	1	1.99	2	1.79	19	1.79	20	1.78	19
Ratio post-sig. to sig.	.80		1.01		.93		.98		.92	

clear Form IV caused the post-significant records to contaminate and weight too heavily the non-significant records, while the bunching of the *significant* stimuli in the last half of the test in Form V may have served to free the great majority of the irrelevant stimuli reactions (including the *post-sigs.*) from the unusually large degree of cautiousness which was precipitated by the significant stimuli in the *irregular 50:50* distribution in Form IV.

III. SUMMARY

1. This investigation has involved the testing of 49 male subjects, through five trials each, with the word-association reaction-time experiment. The results as computed and discussed appertain to reaction-time data alone, ignoring the peculiarities of the word-responses themselves.

2. The comparative differences, as revealed in reaction-times, between five word orders of the relevant stimuli have been measured, compared, and contrasted. The five orders employed were: the *"center-bunch,"* the *"split-bunch,"* the *"interspersed-regular,"* the *"9-to-11 (50:50) irregular,"* and the *"3-to-2-to-6-to-9 (25:75) irregular."* These word orders have been called Forms I, II, III, IV, and V, respectively.

3. Forms IV and V have shown themselves to be radically different from each other; and Form IV possibly to be most like the other three forms. Forms I, II, and III possess not inconsiderable similarities.

4. Despite preliminary expectations in which Form V was anticipated to be the most and Form IV not the least efficient test, judging solely by the reaction-time data, no one form has proven to be unequivocally the most efficient. On certain criteria Form V ranks well; on others, Form IV is recommended; and on one or two criteria Form IV ranks very low indeed.

5. The facts on which these conflicting selections rest have been the following: (1) Form V shows the biggest difference, by the method of correlation between rank orders of subjects, between the reaction-times to the relevant and the irrelevant stimuli, and Form IV the smallest difference. (2) Form V exhibits the greatest kinship with each other form for reaction-time to the irrelevant stimuli alone, and Form IV the greatest disparity. (3) Form V correlates least highly with each other form for reaction-time to relevant items, and Form III the most highly. (4) Differences between the correla-

tion coefficients, as between the significant and irrelevant items in each form, and the probabilities that these are not chance differences, show that Forms V and IV are most unlike each other and also most unlike the other forms. (5) Differences between the correlation coefficients, as between the forms in the matter of reaction-time to the relevant items alone, and the probabilities that these are not chance differences, reaffirm that Forms V and IV are most unlike each other and are greatly dissimilar to the other forms. (6) Differences between mean scores, as between the relevant and the irrelevant items in each form, and the probabilities that these are not chance differences, are so great as to give a probability of 995 in 1000 for Forms V, I, II, and III, while the difference for Form IV is so low as to give a probability of only 71 in 100. Form IV, therefore, has, on this one criterion alone, produced a difference which is not greatly free of the effects of chance. When absolute mean scores are compared for the significant items directly, ignoring correlation, only small differences appear as between forms. But we have argued that the latter is no proper method of instituting the comparison, since it ignores the differential effects on the various subjects. (7) Form IV was proven to be greatly contaminated in its irrelevant scores by the presence there of strong perseverative effects; this was much less true of Forms V and III, while in this respect it was necessary to ignore Forms I and II. (8) General considerations, taken from the literature and based upon common-sense, suggest that Forms I and II are not desirable because they bunch the relevant items, thus preventing the measurement of perseverative effects and also perventing the surprises and shocks which prove to be useful in this test. The subject can easily, with these forms, be forewarned of the likely presentation of the next critical stimulus. Certainly, Forms I and II do not alternate with sufficient frequency irregular periods of relaxation and disturbance. Form III possesses as its most undesirable characteristic regularity of the appearance of each relevant word. Why Form IV should, on certain criteria, have proven to be inefficient and on others to have been most efficient we cannot state; its chief and only difference from Form V lay in its 50:50 distribution of relevant items as between the first and last halves of the test, apparently a virtue, rather than a defect, not possessed by Form V. It is shown that Form IV's efficiency was somewhat reduced by the post-critical or perseveration effects among its irrelevant items—this being indeed a virtue, according to the logic of the test, if and when the *post-sigs.* are, in the data, separated from the irrelevant stimuli.

6. There was a very evident effect of practice, independent of form, in the nature of reduced reaction-time.

7. This increased speed of reaction was far more characteristic of the relevant items than the irrelevant. The relevant items recurred in all forms and between all trials, while synonyms of the irrelevant items of Form I characterized Forms II, III, IV, and V. Moreover, the relative positions of the *non-sig.* items remained the same, from form to form, while the significant items, though repeated, were repeated in different orders among themselves and with reference to distributions among the *non-sig.* items. The latter condition should have materially reduced the practice effect for the relevant items and increased it appreciably for the synonomous irrelevant stimuli, but such retardations and accelerations are not discoverable in the data.

8. By the fifth trial practice effects in reaction-time had progressed to a point where the subjects were actually quicker in responding to the significant stimuli than they were to the irrelevant words.

IV. HISTORICAL REVIEW

So far as our quest has gone, the literature on the word-association reaction-time experiment is singularly barren of matter appertaining to the effects of the word-order and the temporal distribution of the critical stimuli. [A paper along this line by Wechsler and Jones (126) appeared in 1928, after our experiment had been formulated and planned.] There is the suggestion in this literature of a hold-over or perseverative effect from a relevant stimulus, and, of course, if there is this effect, it will superpose itself on that of the next stimulus in case the latter is also a critical word. The literature, in other relations [such as those which make the blood-pressure and psychogalvanic measurement methods effective, in the work of Landis and Wiley (64), Landis and Gullette (62), Skaggs (107), Blatz (10), Bayley (7), and Patterson (92)], suggests the effectiveness of surprise or shock in aiding the test to reveal evasion, embarrassment, complexes, etc. Hence, if critical stimuli occur in immediate succession, the opportunity to employ surprise is greatly reduced. Again, the literature, along other lines, suggests that in intelligence testing the omnibus test, just because it does place such a premium upon the ability of the subject to readjust his determining tendencies quickly and often, invokes factors (presumably emotional factors or stability factors) other than those of an intellectual order. Hence, the average intelligence tester avoids the omnibus type of arrangement of subtests and their items in a general battery. On the other hand, a strong argument can be proposed for arranging any emotional test in a form approaching that of the omnibus test, just for the purpose of testing emotional stability. As we have employed the association test, especially in Forms IV and V, it is much like an omnibus test by having its irrelevant and relevant items intermingled and by efforts to prevent the subject from anticipating where next he shall be confronted with a critical word-stimulus. According to the literature, and especially the literature dealing with "objective examinations," it is demonstrated that a subject of average or greater intelligence (as possessed by college students) very quickly becomes aware of the order or system or regularity with which questions are arranged for his perusal and answer, so that he is very clever at "beating the game" if such a system (as in the yes-no, true-false, type of examination) is too regular or otherwise too simple. Also, we believe the literature will bear out the contention that in all work on thresholds random presentations of stimuli, in preference to ascending series alone, or descending series alone, or *regular* alternations between low and high values of the stimulus, are the most efficient seriations to employ. It is an unquestioned fact that practically all subjects are sensitive to rhythmical presentations, and rhythm means grouping and regularity of groups. Hence, if one is to tap complexes—if one is to expose the concealed—if one is to cause the subject to betray himself and reveal "his hand"—it becomes urgent to create surprises and shocks, to avoid rhythms and other regularities of the order of the stimuli, to measure perseverative effects, and to work fast in order to allow the smallest opportunity for recomposure and for the planning of defenses.

As will be recalled, the word-association reaction-time experiment was originated by Francis Galton (32) in 1879. In 1880 Wundt (135) was employing it at Leipzig. Buccola (11) in 1881 published a paper describing his results with it. In 1883 Trautscholdt (118), under Wundt, published a paper on it. Von Tschisch (120) in 1885 published results from the application of the method to a case of paranoia and a case of progressive paralysis. Cattell described in 1887 work he had begun in Leipzig in 1885, and in 1889 he published (18) a second paper. From 1891 to 1896 Jastrow (50), Calkins (15, 16), and Tanner (115) were involved in heated argument as to sex differences as revealed by the test. And in 1896 Aschaffenburg (2), somewhat after the lead of von Tschisch, published the results of the application of the test to psychopathic subjects. He continued these publications (3) through 1899 and 1904. In 1898 Ziehen (137) applied the method to contrast children with adults. In the momentous year of 1904 there appeared the paper of Wertheimer and Klein (131), describing the success of applying the method to the problem of diagnosing guilt or innocence. In 1905 (and certainly not earlier than 1903, as far as the reviewer can ascertain) there appeared Jung's (54 or 53) first reference to the method. It has been he and his co-workers who have most extensively employed the method for investigating psychopathic subjects and in unearthing emotional complexes. Their efforts have been so extensive as to cause many people to believe that the method was originated by Jung and that it has no applicability except in the field of psychopathology, opinions which are, according to the literature, wholly unfounded and misleading. Jung's investigations continued and culminated in a book (58) which was translated by Eder in 1919. In relation to the employment of the method to guilt detection, one should not forget the names of the German, Grabowsky (37, 38, 39), Alfred Gross (41), Jung himself (54-58), Kramer (111), Ritterhaus (98), Stern (111), Lipmann (67*a*), and Benussi (8). In our own country, such names as Münsterberg (86), Yerkes and Berry (136), Washburn and Leach (121), Henke and Eddy (44), Burtt (14), Marston (73, 74), Crane (20*a*), and Stumberg (112) have loomed large in the matter of guilt detection. The foremost investigator of deranged associations in abnormal persons in the United States has been Rosanoff (60). The method has also been applied, in this country, to measuring intelligence, notably by Rosanoff and co-workers (26*a*, 84, 99), E. E. Jones (52), Otis (89), Kelley (59), Woodrow and Lowell (134), Conrad and Harris (19), and McFadden (80). In England intelligence has been tested in this manner by Rusk (100), and Burt and Moore (13). Important criticisms of the method have appeared under the names of Isserlin (48), Schnitzler (102, 103), Scholl (104), Ranschburg (94), Wigmore (132), Crane (20*a*), and Sutherland (114). The chief fault which May (76) has found in the work of Crosland (21) in his application of the method to measuring deception was the latter's inability to procure more than 11 guilty or suspected subjects, a defect which Crosland and Beck (22) have tried to remedy in a later investigation in 1931. H. T. Moore (85) employed the method for testing the strength of instincts; Ekdahl (27) measured attitudes with it; and, more recently, Cox-Miles and Terman (82, 83) have been engaged in measuring traits of femininity in men and masculinity in women.

That the different parts of speech, serving as stimuli, have different effects on reaction-time has been abundantly demonstrated. For example, Cattell (18) demonstrated that abstract nouns lead to the slowest reaction-time, followed in order by verbs, and then concrete nouns which produce the fastest reaction-time. Jung and Riklin (58) employed, in a list of 400 stimulus-words, 231 nouns, 69 adjectives, 82 verbs, and 18 adverbs and numerals, but failed to state what were the differential effects on reaction-time induced by the different parts of speech. Wehrlin (58), however, working with imbeciles and idiots, rather than with normals, found that the fastest time occurred to verb stimuli rather than to concrete nouns. Jung (58), working with 158 epileptics, and employing 75 concrete nouns, 25 abstract nouns, 50 adjectives, and 50 verbs as stimuli, again failed to present any results which accrued from the parts of speech employed. He showed subsequently, however, that his 26 normal, educated and uneducated men and women subjects exhibited faster reaction-time to concrete nouns than to either verbs, adjectives, or universal concepts; that the latter resulted in the slowest reactions. In case the subject responded to a constant type of stimulus, *with* different parts of speech, his reaction-times differentiated themselves in this order: adjective responses, 1.65 secs.; verb responses, 1.66 secs.; concrete noun responses, 1.81 secs.; and universal concept responses, 1.98 secs. In his 1909 lecture at Clark University Jung (57) described the employment of a stimulus list, calculated to stimulate all of the usual complexes, in which various parts of speech were intermingled. Rusk (100), working in 1909 on 22 boys, with ages ranging from 7.5 to 14.5 years, employed disconnected abstract terms, disconnected concrete terms, and concrete terms individually separated but connected as between groups. Practically all of these were nouns, and it was discovered that practice cuts down the time much more on the abstract terms than it does on the concrete terms, the calling up of "free" responses to abstract terms being next to the most difficult for the subjects. Yerkes and Berry (136) reported in 1909 their experiments in testing deception. They performed 4 experiments, upon 1 (?), 2, 1, and 2 subjects, respectively. In Part *a* of Experiment 1, they used 30 stimuli, including 7 *sigs.* and 23 *non-sigs.* The former included 5 nouns and 2 modifiers, and the latter was composed wholly of nouns. In Part *b,* of Experiment 1, 22 nouns and 1 modifier comprised the *non-sigs.,* and 5 nouns and 2 modifiers comprised the *sigs.* In Experiment No. 2, in the *sigs.* were 16 nouns, 11 modifiers, and 11 verbs; the *non-sigs.* were composed of 61 nouns and 1 modifier. Experiments 3 and 4 were much like Experiments 1 and 2 except for the employment of 10 *sigs.* to 20 *non-sigs.* and 20 *sigs.* to 30 *non-sigs.,* respectively. Henke and Eddy (44) in 1909 employed almost equally nouns, modifiers, and verbs in three experiments in measuring deception in 2, 3, and 2 subjects, respectively, the stimuli varying in all from 35 to 38, the *sigs.* being almost twice as numerous as the *non-sigs.* Washburn and Leach (167) in 1910 in testing deception employed two sets of 60 stimuli, 20 *sigs.* to 40 *non-sigs.* each. In Test I, there were, as *sigs.,* 13 nouns, 5 modifiers, and 2 verbs. In Test II there were 10 nouns, 6 modifiers, and 4 verbs. The *non-sigs.* contained, in Test I, 37 nouns and 3 modifiers; in Test II, 37 nouns, 2 verbs, and 1 modifier. Kelley (59) in 1913 employed on only 12 subjects 100 stimuli, the majority of which were abstract; and the test being everyday words, he was able to run correlations between certain classes of responses and marks of proficiency in mathematics, science, and foreign languages. The coefficients ranged from as low as +.37 to as high as —.75. Otis (89) in 1915 described the application of the Kent-Rosanoff 100-word list to 130 children at Vineland, New Jersey. She found the dull children slow in responding, and also prone to use more than one word in reply (a result very similar to those of Jung, his co-workers, and Binswanger). Crane (20*a*) in 1915 actually tested the effects of 50 definite nouns, 50 verbs, and 50 modifiers (although his word-list, as printed, contains 205 items) on 30 subjects, as a preliminary to measuring deception. Modifiers gave the fastest reaction-time, 1.35 secs. In his main experiment, on deception, 79 words were used, approximately equal (?) as between nouns, verbs, and modifiers. There were 33 *sigs.* and 46 *non-sigs.* in the list. H. T. Moore (85) in 1916 described his experiment to test the strength of instincts. It involved 20 neutral words in the preliminary and 100 in the main test, mostly all nouns, apparently applied to only 1 subject. There were 10 words per instinct and 10 instincts investigated, there being, therefore, no *non-sigs.* Griffitts (40) in 1920 employed on 36 subjects as stimuli 25 adjectives and 25 nouns, divided equally in each group, to arouse pleasure and displeasure. He discovered that nouns, as stimuli, militate against recall in subsequent tests by the subject of the former stimuli, while adjectives were more prone to aid the recall of them. In 1922 McClatchy (78) reported the effects of 44 general and 44 local, familiar, word-stimuli upon the reaction-times and kinds of responses of 90 undergraduate college students. Naturally, the general stimuli contained many modifiers, while the local words were most largely concrete nouns. Local words led to the slower reaction-time. Variability was greater for the local words. General words led to a narrower range of response in the subjects. It is suggested that general associations, being older and more generally organized, are stronger and hence lead to faster reaction-time. And, again, the necessity of having to choose, as between several responses, to local stimuli, may slow down the reaction-time. Perhaps local stimuli are much more effective in tapping emotions and emotional complexes. In a second study McClatchy (and Cooper) (79) in 1924 ran a free-association test against analogies, proverbs, and sentence-completion tests. They employed on 60 subjects 88 word-stimuli equally arranged as between local and general words. Intellectual equipment and language abilities, as well as emotional inhibitions, affect reaction-time. Sentence-completion time and word-association reaction-time correlate as highly as +.59. Form of word response correlates as highly as +.80 with analogies scores, and form of verbal responses in the association test correlates with proverbs as high as +.76. Stumberg (112), in 1925, in testing the efficacy of the deception test, on sophisticated as against naïve subjects, sought to omit, from the word lists of 25 and 50 words, all unusual or foreign words. He worked with 5 sophisticated and 4 naïve subjects.

Now, with respect to the distribution and localization of relevant stimuli among the irrelevant, as sai dabove, the literature is almost a complete blank. There has been a wide variance in the number of relevant and irrelevant stimuli employed, but even greater variation (and in almost no case supported by either fact or argument) has characterized the placement of the relevant stimuli. Jung (57), in his 1909 lecture at Clark University, came nearer than at any other time to justifying the employment of 100 word-stimuli and their relative placements with respect to one another. He asserted that the list as presented was the result of much labor and repeated experiences in word-association experiments, but he nowhere has stated that his word order was intentional, nor has he given the reasons for it. He did, however, in 1909, treat his relevant, irrelevant, and post-significant stimuli separately, both for the time records and word responses. Hull and Lugoff (47), in 1921, in analyzing Jung's list and word order experimentally were able to rank the 20 words which gave the highest number of signs of diagnostic potency; these, with one exception, turned out to be words which one would normally call relevant; hence, Jung must have designed them for that purpose. They proved to be effective on the 100 men and women on whom these investigators worked. The exception was a word which no subject understood, the word "supplicate." They also discovered that Jung must have deliberately followed each "strong," *significant*, word with a "weak" one, with an *irrelevant* one, and that alternately between the "strong" words + and — words (in directions of potency) of "weak" character were placed as many as five items away from each "strong" in both directions, above and below. Yerkes and Berry (136) employed in all 82 relevant and 158 irrelevant stimuli, and inclined toward grouping the *sigs.* in groups of consecutive stimuli, without a break between four such words. They justify neither the ratio of *sigs.* to *non-sigs.* nor the manner of distribution and placement. Henke and Eddy (44) employed in all 34 *non-sigs.* and 73 *sigs.,* bunching most of the *sigs.* at the bottom part of each list. They are, likewise, just as remiss in failing to justify either the proportion of types of stimuli or the word order. Washburn and Leach (121), in two main experiments, employed 60 stimuli each, divided into 40 *non-sigs.* and 20 *sigs.* The *sigs.* were rather grouped into consecutive arrangements of 3 or 4 words each, to "increase their disturbing effect." Other than the small groups, the stimuli were scattered throughout the whole list, a majority of them being placed in the last half of the test. The latter, however, they did not justify. Rusk (100), by employing a technique involving connected and disconnected words and word groups approached the problem but did not effect different distributions of stimuli and different word orders. Otis (89) and Kelley (59) appear not to have been cognizant of word-order effects. Crane (20*a*), though measuring the effects of word order, for certain well-known word sequences, such as "ham and ——," did not approach the problem of the distribution of the *sigs.* within the body of the *non-sigs.* Moore (85) had no *non-sigs.;* hence there was no problem of distribution for him. He did, however, interchange and intermingle the 10 sets of instinct-provoking words, presumably into a random order. Griffitts (40) by running, in his main experiments, only words calculated to arouse either pleasure or displeasure, and further concentrating nouns together and adjectives together, apparently did not consider our problem. Stumberg (112), tending to employ *sigs.* in pairs, with a *non-sig.* between pairs, and with *sigs.* as numerous as *non-sigs.*, was not concerned with the problem of word order or general distribution of stimuli. Hubbard (45), 1924, appears to have been the first investigator to recognize that word order and distribution may be capable of producing serious effects in the word-association reaction-time test. Consideration of Jung's assertion of the fact of perseveration, of two sorts, shown in post-significant word responses and reaction-times, led her to employ 50 chance arrangements of Jung's 100 words. She did this with 50 men and 50 women subjects, and thus was able to measure the effects of stimuli situated in progressive and successive twelfths of the whole 100 words, from the top of the test to its bottom. She employed 8 diagnostic signs. Perseveration, just after a relevant word, expends its force almost completely by the time the fifth (fourth post-critical) word has been reached. This reduction is also very sharp and precipitous. On the basis of 8 words in a group, 12.5 groups in 100 words, the tendency of the subject to laugh grows progressively less from the 1st to the 13th group (beginning to end of the test). This decline in the likelihood of laughing is most precipitous as between the first and second groups of 8 words. Hence, she regards laughter in the last part of the test as being more symptomatic of emotional disturbance than in the beginning of the experiment. Similarly, failure to recall past responses gradually grows less from the beginning to the end of the test, with slight and irregular rises just beyond the middle of the test. Both the number of abnormally long reaction-times and the average length of all reaction-times increase sharply from the beginning of the test through the second group of 8 words; thence, there is a slow but steady rise in both, until a small decline sets in after the 40th word. From there on, this decline never approaches the fast reaction-times characteristic of the first 8 words of the test. And, in somewhat converse fashion, the tendency to respond with individual responses declines to the 16th word and then begins a somewhat steady rise from which it never again drops. From all of which the present investigators have deduced that the last half of the test is much more useful for diagnostic work than the first half, because in general even normal and innocent subjects here possess less self-control and poise and are more or less distraught.

In 1929 Crosland (21) published his results from measuring deception in students charged with stealing. He had tested 11 suspects and 34 innocent, control subjects. He employed 100 words (not all common nouns, however), including 20 relevant and 80 irrelevant stimuli. He segregated the responses and time records, especially the latter, into 20 significant, 19 post-significant, and 61 irrelevant items. The 6th, 11th, 15th, 22nd, 26th, 35th, 39th, 45th, 52nd, 60th, 64th, 70th, 73rd, 80th, 83rd, 88th, 92nd, 95th, 98th, and 100th positions were occupied by relevant stimuli. Each of the immediately succeeding positions he called the post-critical positions. All other positions were occupied by stimuli called irrelevant. Scrutiny of the foregoing distribution will reveal that no two *sigs.* came immediately together; that the number of intervening irrelevant items is irregular or varies; that 12 *sigs.* occur in the last 50 words and only 8 in the first 50; and that the *sigs.* have a ratio to the *non-sigs.* of 1:4. All of these matters resulted from design. After years of employment of the method in the laboratory, he was convinced that 20 items was the smallest number of samples one should seek to obtain. Likewise, it appeared that a much larger number of *non-sigs.* would be necessary, to hide and block off the *sigs.*—to create a condition to disarm the subject, to present surprises to him, and to make use of irregularity of intervening *non-sigs.,* to further the disarming process, and to aid in the creation of surprises. With design, also, the arrangement to make use of the subject's distraught condition, during the last half of the test, was planned, to concentrate more than half the *sigs.* there. It was his opinion also that future investigations could well devote themselves to the study of sexual complexes, because most students experience embarrassment even in this modern day in discussing the question of sex. Hence, perhaps, an investigator, employing words related to sex, could procure a sufficient number of subjects who are "guilty," that is possess conflicts, inhibitions, embarrassment, etc., to meet the fewness of cases (11) which was the worst fault of his 1929 monograph on measurements of deception [Crosland and Beck (22) have recently carried out this plan].

Wechsler and Jones (126) have discovered that at least five "buffer" words or *non-sigs.* should interpose between two significant items, but a *sig.* will provoke its effect no matter where it is. If the experimenter wishes a contrast between the *sigs.* and the *non-sigs.,* the system should employ "buffer" words between the *sigs.* Also large individual differences require that the same word order be applied to all subjects.

Several investigators, but especially Veraguth (119), Binswanger (9, 58), Jung and Peterson (93*a*), Rusk (100), Wells (128), Griffitts (40), and Crane (20*a*) have discovered practice effects in the word-association reaction-time experiment, consisting, among other effects, of greatly reduced time or increased speed of reaction.

May (75) and perhaps others have shown that foreperiod "preparation" serves to diminish reaction-time. This result is not unrelated to the reasons which have throughout this paper been insisted upon as grounds for controlling the word order and the distribution of the relevant stimuli . The sensitiveness of the usual subject to rhythm or any system of regularity is not unrelated to his susceptibility to the influence of "preparation." And, lastly, it has been argued that devices should be employed in the test to enable the disarming and the surprising of the subject, to make the test more effective, just because "preparation" is so universally recognized to be effective in determining reactions and the speed with which they occur.

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LES EFFETS SUR LE TEMPS DE RÉACTION DE DIVERS ORDRES ET DISTRIBUTIONS DE STIMULI RELATIFS DANS L'EXPÉRIENCE D'ASSOCIATION DE MOTS

(Résumé)

L'expérience a mesuré les effets sur le temps de réaction de 5 ordres de distribution de 20 mots relatifs au sexe dans une liste de 80 mots non relatifs. Les 5 ordres ont été: I, le *groupe central,* de 40 stimuli insignifiants, de 20 stimuli signifiants, et de 40 stimuli insignifiants; II, le *groupe séparé,* de 20 mots non relatifs, de 10 mots relatifs, de 40 mots non relatifs, de 10 mots relatifs, et de 20 mots non relatifs; III, le *groupe régulièrement entremêlé,* de 4 mots non relatifs et le cinquième stimulus un mot relatif, dans le test entier de 100 mots; IV, le *groupe irrégulier 9:11,* d'une distribution irrégulière des 20 mots signifiants parmi les 80 mots insignifiants, où deux des mots signifiants n'ont pas été placés ensemble et avec une division approximé de 50:50 des stimuli relatifs dans la moitié supérieure et la moitié inférieure du test; et V, le *groupe irrégulier 3:2:6:9,* exactement comme la distribution IV, excepté qu'il se trouve le quart des stimuli relatifs dans la première moitié du test et les trois quarts dans la dernière moitié. On a appliqué cinq grands critères d'efficacité aux divers ordres de distribution. Comme résultat, les Formes IV et V se sont beaucoup distinguées parmi les trois autres Formes, et ont montré des différences signifiantes entre elles-mêmes. Pour quelques critères, la Forme V a été la distribution la plus efficace, surtout pour montrer la corrélation la moins élevée entre ses distributions relatives et non relatives. Pour les autres critères, la Forme IV a montré la plus grande efficacité. Le principal défaut de la Forme IV a été son temps réduit de réaction aux stimuli signifiants et son temps augmenté de réaction aux stimuli insignifiants. Et le principal défaut de la Forme V a été le fait que ses mots non relatifs, dans la dernière moitié du test, ont été influencés par le nombre de mots relatifs près d'eux. Nulle forme n'a été supérieure pour tous les critères d'efficacité.

SWARTZ ET CROSLAND

DIE EINWIRKUNGEN VERSCHIEDENER RANGORDNUNGEN UND VERTEILUNGEN TREFFENDER (RELEVANT) REIZE IN DEM WORTASSOZIATIONSVERSUCH

(Referat)

In diesem Versuch sind die Einwirkungen 5 verschiedener Verteilungsanordnungen (orders of distribution) von 20 sich auf geschlechtliche Fragen bezienden Wörtern (sex words) innerhalb einer Liste von 80 untreffenden (irrelevant) Wörtern gemessen worden. Die 5 Rangordnungen waren folgende: I, eine *Zusammenbüschelung* (bunching), wobei zuerst 40 unbedeutende, dann die 20 treffenden, und dann wieder 40 unbedeutende Reize kamen; II, eine *Spaltung,* wobei zuerst 20 unbedeutende, dann 10 treffende, dann 40 unbedeutende, dann wieder 10 treffende und schliesslich wieder 20 unbedeutende Reize kamen; III, eine *regelmässige Untermengung,* wobei, die ganze Serie von 100 Wörtern hindurch, immer 4 unbedeutende Reize von einem treffenden Reiz gefolgt wurden; IV, eine *9:11 unregelmässige Verteilung* der 20 treffendem Wörter unter den 80 unbedeutenden Wörtern,

wobei nie zwei treffende einander folgen und wobei die treffenden Wörter ungefähr gleich zwischen der oberen und der unteren Hälfte der Prüfungsliste verteilt sind; und V, eine *3:2::6:9 unregelmässige Verteilung.* Diese gleicht fast vollkommen der Verteilung IV; nur findet sich ein Viertel der treffenden Reize in der ersten Hälfte der Prüfungsserie und finden sich drei Viertel in der zweiten Hälfte. Es wurden fünf Hauptkriterien der Wirksamkeit auf die verschiedenen Anordnungen der Verteilung angewendet. Man fand, dass die Anordnungen IV und V sich hervorstechend von den drei anderen Anordnungen auszeichneten und auch bedeutende Unterschiede von einander erwiesen. Vom Standpunkt einiger Kriterien erwies sich Anordnung V als die erfolgreichste, besonders darin, dass sie die niedrigste Korrelation zwischen ihren treffenden und ihren unbedeutenden Verteilungen lieferte. In Bezug auf die übrigen Kriterien erwies die Anordnung IV die höchste Wirksamkeit. Der Hauptdefekt der Anordnung IV lag in ihrer verminderte Reaktionszeit auf die unbedeutenden Reize; und der Hauptdefekt der Anordnung V bestand darin, dass ihre unbedeutenden Bestandteile, in der zweiten Hälfte der Prüfung, direkt durch die Zahlreichheit der treffenden sie umgebenden Bestandteile beeinflusst wurden. Keine der Anordnungen stand in Bezug auf alle Kriterien der Wirksamkeit am höchsten.

Swartz und Crosland

SHORT ARTICLES AND NOTES

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MASSED VERSUS DISTRIBUTED PRACTICE IN PURSUITMETER LEARNING[1]

Walter W. Webb

Considerable experimentation has been done on the problem of massed versus distributed practice. Various workers have claimed the superiority of one method or the other. For example, Culler (1), in an experiment using the Hampton Court pencil maze, advocated the use of massed practice periods on the basis of his results. Warden (9), experimenting with rats, found evidence favoring the massing of practice. However, Harder (2), in a comparative experiment using humans, got quite different results, and concluded that any optimal distribution depends upon the type of organism serving as subject. Pyle (5), in his experiment on the learning of typewriting, found that "the distributed practice is best, but concentrated practice brings in good returns, and if one is in a hurry to acquire a skill, the diminished returns for concentrated practice need not be considered prohibitive." Lashley (3), in his work on the acquisition of skill in archery, found that distributed practice was best. However, he advocated the massing of practice over the last half of the period for the greatest economy in learning.

While some experimenters favor massing, the majority favor some sort of distribution of practice. Ruch (8), in reviewing the literature on massed and distributed practice in learning, states that those experiments "involving motor learning indicate that distribution is most economical in avoiding errors rather than in making for rapid learning."

In this paper I shall present a comparison of massing versus distributing practice in learning to operate a pursuitmeter. The results should throw further light on the controversy outlined above.

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[1] The writer wishes to acknowledge the assistance given him by Dr. Samuel Renshaw in the conduction of this experiment.

THE APPARATUS

Miles (4) points out that "the nature of the pursuit task not only requires quick reaction but also that the movements be executed in a fairly definite pattern. A reaction is usually only the beginning of coordinated movements toward some end or series of such coordinations, and probably in most instances in practical life the adequacy with which the series of coordinations is carried through is fully as important, if not more so, than the mere matter of speed in initiating them. Especially would this appear to be the case in movements for compensating, directing aiming, or otherwise tending any moving object where the pace and the pattern are not set entirely by the subject himself."

This type of serial motor action test is more closely associated with conditions of human performance and behavior than are those tests of single-reaction type. The operation of the Renshaw pursuitmeter requires a rather unique pursuit task. The subject is required to maintain contact on a moving bead by holding a stylus wired in an electrical circuit to a terminal in the same circuit and located in the bead at the end of a moving bead-bar. As long as this contact between the terminal located in the hammer-shaped stylus and the terminal located in the bead at the end of the bead-bar is maintained the apparatus is kept moving. When the subject breaks this contact he also breaks the electrical circuit. The machine loses speed and will come to a complete stop in 4 seconds if the contact is not re-established. The subject's lost contacts or errors are recorded on the Renshaw-Weiss polygraph (7). The field of movement of the bead over a brass plate is very complicated and irregular. It moves in a diamond-shaped area of 4 by 5 inches. The bead-bar is driven by a system of four small gears connected to the bar by a series of double-toggle levers. All that the subject sees, as he operates the instrument by maintaining this contact, is the stylus, the bead moving irregularly over the brass plate, and part of the bead-bar. A detailed description of the Renshaw pursuitmeter has been given in an earlier publication (9).

THE METHOD

Fourteen men and 10 women, divided into four groups of subjects, ran the pursuitmeter for 36 cycles. Each cycle was 4 minutes long, not including the time consumed by lost contacts. One sitting consisted of three cycles. All subjects were permitted rest periods of 3 minutes between each cycle. This rest period was not an idle one, since the subject was asked to write out descriptive protocols setting forth as accurately as possible the mode of attack, his conception of his own success, changes introduced in the course of the practice series, and the like. Group I (see Figure 1) consisted of four subjects who ran one sitting weekly for 12 weeks. Group II was made up of 4 subjects who ran three sittings a week for 4 weeks.

Group III consisted of 8 subjects who ran one sitting daily for 12 days. Group IV was made up of 8 subjects who ran the 12 sittings continuously (i.e., during one afternoon), except for their rest periods.

The subjects were told to seat themselves comfortably on a high stool, hold the stylus as they thought best, and keep the motor running by maintaining the contact of the stylus on the bead. When they saw a small light before them flash on, it indicated that they were not on contact. They were told to analyze their procedure, seeking a "best" method. This type of instruction was used because Renshaw and Postle (6), in an experiment with this same apparatus, found that subjects gave the best performance if not given complete instruction but told to analyze their procedure so as to find the most effective method of keeping the motor running. The practice periods of 36 cycles were plotted for each individual. In Figure 1 are plotted the means of the four experimental groups. On the abscissa 12 points are laid off, each representing a sitting in the order run. The ordinate represents the mean number of errors or lost contacts per sitting.

The Results

Figure 1 shows the four experimental groups whose time distribution ranges from continuous running to one sitting a week. This represents a distribution of time from those who massed their practice in 6 hours to those who distributed their practice over a period of 2016 hours (12 weeks). The longest learning period for any individual was 336 times the shortest in elapsed time. Those subjects who ran three sittings weekly (Group II) had the best learning curve for the first half of the entire series, that is, for six sittings. Then, for the brief space of one sitting, those who ran daily (Group III) were best in performance. From this point to the end of the series those who ran but one sitting a week (Group I) were ahead, except for a slight superiority of Group III in the ninth sitting.

From this one might assume that the best distribution would be a composite time distribution involving the first half of the series to be run three sittings a week, the next portion to be run daily, and the last four or five sittings to be run once a week. From his survey of similar experiments, Ruch (8) concludes that "the possibility exists that distribution may be more effective during the initial stages of habit formation or learning of any sort and massing more efficient in the final stages, or vice versa, in which case a combination of a greater degree of distribution at one period and a lesser degree at others would produce the optimal arrangement of work and rest periods." In this experiment a wide divergence through chance is shown for the groups in the first sitting. Here the results had no chance to be affected by any time distribution. Ruch (8) points out that "during the first periods in acquiring a new motor skill without instruction, there are many sudden improvements through 'insight' which are unrelated to distribution, which accounts possibly for the failure between spacing of practice and improvement to appear." When one observes that the terminal points of all the group means approach an asymptotic limit it is apparent that the time distribution factor has but little influence on the ultimate results.

Does the fact that those who ran the series continuously and had the

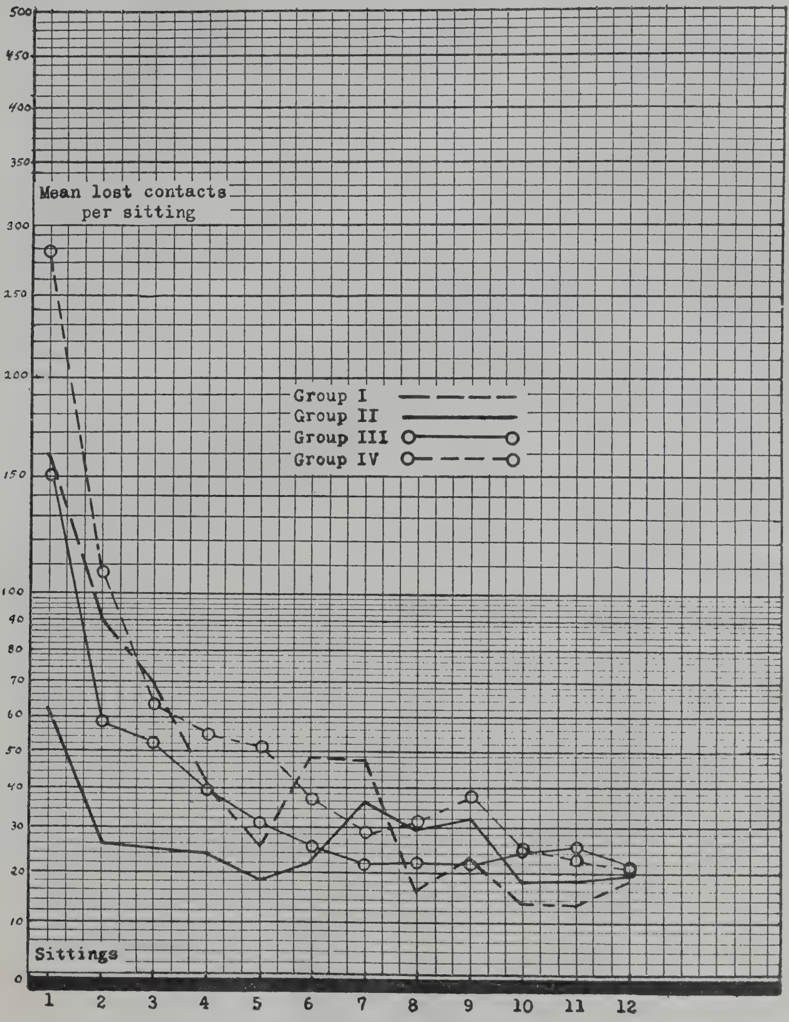

FIGURE 1

greatest drops of all the groups for the first three sittings mean that this group learned more rapidly than the rest of the subjects? If we disregard the first three sittings there is little difference in the amount or rate of learning between the groups. Any difference that is present can be charged to sampling and individual differences within the groups.

Group II (three sittings weekly), which started with a mean that is much lower than those of the other groups, contained members of a class in experimental psychology who had heard a discussion of the apparatus. There is little doubt that this group had extra motivation since motion pictures were taken of their runs. The instructor was present. This may have been another factor which stimulated the subjects to do their best. Despite these factors, this group soon fell in step with the others and ended up at the same point. It might be said that this group really learned the least of all the groups in that they started so much lower than the others. According to such a criterion, Group IV would be considered the best learners as they started much higher than the rest but ended at the common terminal point. Of course, they also might have had more to learn.

We could term those who ran one sitting weekly, Group I, the most consistent if it were not for Sittings 6 and 7. This peak can be traced back to two women subjects who had consecutively high error scores. The peaks, found so much in the individual curves, are quite characteristic of this type of experiment. They represent regressions such as: a change in method, a blocking of successive discriminatory processes, outside stimuli disturbing the subject, attempted domination of the apparatus, and any transitory emotional factors such as anger.

The continuous group (Group IV) reveals an interesting angle of the problem of fatigue. If fatigue is cumulative, as some think, it would be a question as to what rest periods within the group were most needed to overcome or minimize this factor. Group IV, while starting with a greater number of lost contacts, drops into line with the others and ends up at the common terminal point. Fatigue in such continuous running appears to have little effect on the ultimate result. Of course, the matter of fatigue is difficult to differentiate from the many other factors which have shown tendencies to disrupt the learning process or deprive the subject of the full benefits of the practice period. This would seem to indicate that this type of learning is ultimately dependent upon the reaction pattern of each individual. These variations within each group make it difficult to say that any of the learning curves are typical of the groups. There are numerous methods of attacking the pursuit task, some being much better than others. The subject who hits upon one of these better methods, either by analyzation or by accident, has the best chance of acquiring this particular skill in the quickest and most efficient way so far as his physical characteristics are conducive to such learning.

Summary and Conclusions

An attempt was made to find an optimal time distribution for the acquisition of skill on the Renshaw pursuitmeter. Twenty-four persons, 14 men and 10 women, learned to run this apparatus. The task to be learned required the following of a moving bead over a brass plate. The field of movement was very irregular. The subject held the stylus on the bead so that an electrical circuit was closed as long as contact on the terminal in the bead was maintained, the stylus being wired as part of the circuit. Breaks in the circuit were recorded as errors or lost contacts. Learning curves were plotted for four groups of varying time distribution.

The results show that all the groups arrived at practically the same point after running the pursuitmeter for 36 cycles. It is apparent that, regardless of frequency and time distribution, the groups arrive at the same point (see Figure 1). While there are variations in the curves, they are not large enough to indicate any significant difference in the time distributions. There is revealed no essential difference between those who massed their practice into a period of 6 hours and those who distributed their practice over 2016 hours or 12 weeks. All the subjects ran the pursuitmeter the same number of cycles, 36. The longest learning period was 336 times the shortest in elapsed time. From this it is evident that the subject who practiced over a 6-hour period got just as much training in the acquisition of this skill as the subject who practiced over a period of 12 weeks. Frequency and time in this experiment are not as important as the frequency of correct responses given. While the possible attack methods are very numerous, there are but a few "best" or correct responses. If either by analyzation or by accident, one of these are used from the start, learning will take place more rapidly.

Contrary to many of the findings of previous experiments on massed and distributed practice, we conclude:

1. There is no significant difference between massed and distributed practice as far as the acquisition of skill on the Renshaw pursuitmeter is concerned.
2. There is evidence in the experiment supporting the theory that a composite time distribution might be best for the acquisition of a skillful act.
3. The mode of attack seems to influence the rate of learning to a greater extent than changes in conductivity of nerve and muscle.

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ANOTHER SHORT METHOD FOR CALCULATING β WEIGHTS AND THE RESULTING MULTIPLE R ON A COMPUTING MACHINE[1]

Cecil R. Brolyer

If one wishes to express one variable as a linear function of n other variables, it is necessary somehow to get the equivalent of a solution of at least n equations in n unknowns. These equations are the so-called *normal equations*, and they may be written immediately from a knowledge of the correlation coefficients, if *standard scores*[2] are used.

If one is dealing with a criterion and five other variables the essential problem is the determination of the five a's or β weights of Equation 1 which will make $\Sigma(z_0-\bar{z}_0)^2$ a minimum where z_0 is the measured criterion and $\bar{z}_0$ is the criterion expressed as a linear function of the five other variables.

$$\bar{z}_0=a_1z_1+a_2z_2+a_3z_3+a_4z_4+a_5z_5 \qquad [1]$$

The five normal equations for the solutions of the above a's are obtained by multiplying Equation 1 (after z_0 has been substituted for $\bar{z}_0$) by z_1, z_2, z_3, z_4, and z_5 in succession, summating, and dividing by the number of cases. For this number of variables the equations would be:

$$\begin{array}{llllll}
a_1 & +\,a_2r_{12} & +\,a_3r_{13} & +\,a_4r_{14} & +\,a_5r_{15} & -\,r_{01}=0\\
a_1r_{12} & +\,a_2 & +\,a_3r_{23} & +\,a_4r_{24} & +\,a_5r_{25} & -\,r_{02}=0\\
a_1r_{13} & +\,a_2r_{23} & +\,a_3 & +\,a_4r_{34} & +\,a_5r_{35} & -\,r_{03}=0\\
a_1r_{14} & +\,a_2r_{24} & +\,a_3r_{34} & +\,a_4 & +\,a_5r_{45} & -\,r_{04}=0\\
a_1r_{15} & +\,a_2r_{25} & +\,a_3r_{35} & +\,a_4r_{45} & +\,a_5 & -\,r_{05}=0
\end{array}$$

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[1] After this article was in manuscript, the writer read still another method of solving such equations. See Horst (1).

[2] A *standard score* is of the form $\frac{X-M}{\sigma}$. After Equation 1 has been obtained, this substitution may be made if it is desired to have the expression in terms of raw scores.

The method consists in eliminating one unknown after the other from equations in one of which the coefficient of the variable to be eliminated is unity. The five normal equations and their solutions are as follows:

$$\bar{z}_0 = a_1 z_1 + a_2 z_2 + a_3 z_3 + a_4 z_4 + a_5 z_5$$

$$\begin{array}{rrrrrrl}
a_1 & +\ .5225a_2 & +\ .4432a_3 & +\ .4849a_4 & +\ .2590a_5 & -\ .3494 = 0 & 2.3602 \\
.5225a_1\ + & a_2 & +\ .4422a_3 & +\ .5805a_4 & +\ .2840a_5 & -\ .4980 = 0 & 2.3312 \\
.4432a_1\ + & .4422a_2\ + & a_3 & +\ .4916a_4 & +\ .2264a_5 & -\ .4883 = 0 & 2.1151 \\
.4849a_1\ + & .5805a_2\ + & .4916a_3\ + & a_4 & +\ .3711a_5 & -\ .4785 = 0 & 2.4496 \\
.2590a_1\ + & .2840a_2\ + & .2264a_3\ + & .3711a_4\ + & a_5 & -\ .3862 = 0 & 1.7543
\end{array}$$

$$\begin{array}{rrrrrl}
\underline{1.3914a_2} & +\ \underline{.4031a_3} & +\ \underline{.6261a_4} & +\ \underline{.2845a_5} & -\ \underline{.6037} = 0 & \underline{2.1014} \\
a_2 & +\ .2897a_3 & +\ .4500a_4 & +\ .2045a_5 & -\ .4339 = 0 & 1.5103 \\
.4752a_2 & +1.8131a_3 & +\ .6243a_4 & +\ .2518a_5 & -\ .7524 = 0 & 2.4121 \\
.6747a_2 & +\ .5706a_3 & +1.5774a_4 & +\ .5063a_5 & -\ .6374 = 0 & 2.6916 \\
.5740a_2 & +\ .4309a_3 & +\ .9479a_4 & +\ 3.6020a_5 & -1.1417 = 0 & 4.4132
\end{array}$$

$$\begin{array}{rrrrl}
\underline{3.5257a_3} & +\ \underline{.8638a_4} & +\ \underline{.3254a_5} & -\underline{1.1494} = 0 & \underline{3.5657} \\
a_3 & +\ .2450a_4 & +\ .0923a_5 & -\ .3260 = 0 & 1.0113 \\
.5560a_3 & +1.8879a_4 & +\ .5459a_5 & -\ .5108 = 0 & 2.4790 \\
.4610a_3 & +1.2014a_4 & +\ 6.0708a_5 & -1.5551 = 0 & 6.1782
\end{array}$$

$$\begin{array}{rrrl}
\underline{3.1505a_4} & +\ \underline{.8895a_5} & -\ \underline{.5927} = 0 & \underline{3.4473} \\
a_4 & +\ .2823a_5 & -\ .1881 = 0 & 1.0942 \\
2.3611a_4 & +13.0765a_5 & -3.0473 = 0 & 12.3904
\end{array}$$

$$\begin{array}{rrl}
5.2560a_5 & -1.1025 = 0 & 4.1535
\end{array}$$

$$a_5 = .2098$$

$$\begin{aligned}
a_1 &= -.0217 \\
a_2 &= .2533 \\
a_3 &= .2751 \\
a_4 &= .1289 \\
a_5 &= .2098
\end{aligned}$$

$$\begin{aligned}
r_{0.12345} &= \sqrt{.3956} \\
&= .63
\end{aligned}$$

The present short-cut was discovered in using a Monroe Calculating Machine, Model KASC-163, to solve *normal equations* by the Doolittle Method (3, p. 577). The short-cut consists essentially in noticing that the above type of machine will give at once solutions to such expressions as $\left(\frac{e}{f} - g\right)$. As a matter of fact, it is necessary only that the machine be of sufficient size to give the desired accuracy of computation and that the windows showing the results of the division be connected with each other by a carry-over device, though it is more convenient if the machine has two sets of such windows, which can be made to show complementary numbers and of which one set can be retained while the other is cleared. To obtain the solution of $\left(\frac{e}{f} - g\right)$, the only requirement is that, in the set of windows which ordinarily shows the quotient, there be the complementary number of g at the time e is divided by f.

The first variable to be eliminated is a_1. The machine is set for division and so adjusted that the right-hand upper set of windows may be retained while the left upper set is cleared. It is generally most convenient if the same number of decimal places is put both in the keyboard and the upper sets of windows. The number .5225, in red, is put in the left-hand upper set of windows. The number 1.0000 is placed in the keyboard and turned into the lower set of windows by depressing the positive bar once. The keyboard is cleared and the negative bar is depressed once to readjust the right-hand upper set of windows to the complementary number of .5225, and the left-hand upper set of windows is cleared. .5225 is placed in the keyboard, and the machine is started on automatic division. The number 1.3913 appears in the upper right-hand set of windows, but this must be raised one figure in the last place as the remainder—the number in the lower set of windows—is greater than one-half of the divisor. (The first equation in the second set of equations is underlined because this equation is divided by 1.3914 to make the coefficient of a_2 unity, and the transformed equation is given immediately below. With this type of machine this is most easily done by setting the machine for multiplication, putting 1.3914 into the keyboard and obtaining the necessary numbers in the upper windows by which 1.3914 must be multiplied to obtain the coefficients of the equation in question. In order not to interfere with the routine machine computation of the coefficients of all a's in any one set, this operation is performed after all coefficients of this set have been obtained.) The keyboard and the lower set of windows are cleared. The left-hand upper set of windows is caused to become zero by depressing the positive bar the proper number of times for each window. The number in the right-hand upper set of windows thereby becomes again the complementary number of .5225. .4422 is put in the lower set of windows. The keyboard is cleared, and the negative bar is depressed once to adjust the upper set of windows for the previous operation. .4432 is set into the keyboard and the machine is started on automatic division. The answer .4752 is written as the coefficient of a_2 in the second equation of the second set. The lower set of windows and the keyboard are cleared and the machine is again ready by turning the upper left-hand set of windows to zero. .5805 is put into the lower set of windows, and adjustment is made for this operation by depressing the negative bar once. .4849 is set into the keyboard and the machine is set for automatic division. .6747 is written as the coefficient of a_2 of the third equation in the second set. After clearing and readjusting the machine, .2840 is divided by .2590, and the resulting value .5740 is the coefficient of a_2 in the fourth equation of the second set. The machine is entirely cleared, and the coefficients of a_3, a_4, and a_5 are obtained exactly analogously, starting with the complement of .4432 in the right upper windows and dividing .4422 by .5225, to obtain .4031 as the coefficient of a_3.

The last column of numbers is used as a check upon the calculation at any desired point. In the first set, these numbers are obtained by adding for each normal equation the coefficients with the proper sign. In determining these numbers for succeeding sets they are treated exactly as if they were coefficients of other *a*'s.

After the value of the last *a* has been obtained, at least one equation in each of the preceding sets is solved successively. More may be solved if the computer wishes to have an idea of the amount of inaccuracy produced by the method of computing.

After these weights have been computed it is an easy matter to determine the multiple correlation from the equation:

$$r_{0.123\,\ldots\,n} = [a_1 r_{01} + a_2 r_{02} + \ldots\ldots a_n r_{0n}]^{1/2}$$

This last form may be obtained very easily from the equation:

$$r_{z_0 \bar{z}_0} = \frac{\Sigma z_0 \bar{z}_0}{N \sigma_{z_0} \sigma_{\bar{z}_0}}$$

in which σ_{z_0} is equal to 1, and by remembering that the multiple correlation coefficient is equal to $\sigma_{\bar{z}_0}$ (2, p. 300).

In the given example, it would be necessary simply to multiply into the machine successively the values just preceding the equality by the weights of the respective variables, that is, .3862 by .2098, .4785 by .1289, .4883 by .2751, .4980 by .2533, and .3494 by —.0217. The square root of the number in the machine, .3956, is extracted and the result, .63, is the multiple correlation coefficient.

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A PROBLEM IN MEANING

SHIGETO TSURU AND HORACE S. FRIES

This paper proposes to illustrate an interesting phenomenon and to explain it on principles consistent with gestalt theory. It challenges any behavioristic and associationistic explanation.

Below are 25 pairs of English words. Opposite each pair is the corresponding Japanese pair in Hebbon Romanized spelling. The reader may demonstrate for himself the ease with which the correct member of each Japanese pair is assigned to its corresponding member in the English pair simply by trying to get the "feel" of the words and then guessing. To do

1.	(a)	bird	mushi	(muʃɪ)
	(b)	worm	tori	(tɔrɪ)
2.	(a)	red	midori	(midɔri)
	(b)	green	aka	(ʌkʌ)
3.	(a)	peace	heiwa	(heːwʌ)
	(b)	war	tatakai	(tʌtʌkʌi)
4.	(a)	sweet	karai	(kʌrʌi)
	(b)	bitter	amai	(ʌmʌi)
5.	(a)	fast	hayai	(hʌyʌi)
	(b)	slow	osoi	(ɔsɔi)
6.	(a)	white	shiro	(ʃirɔ)
	(b)	black	kuro	(kurɔ)
7.	(a)	square	shikaku	(ʃikʌku)
	(b)	circle	maru	(mʌru)
8.	(a)	good	warui	(wʌrui)
	(b)	evil	yoi	(yɔi)
9.	(a)	praise	kenasu	(kenʌsu)
	(b)	deprecate	homeru	(hɔmeru)
10.	(a)	far	tooi	(tɔːi)
	(b)	near	chikai	(tʃikʌi)
11.	(a)	soft	katai	(kʌtʌi)
	(b)	hard	yawai	(yʌwʌi)
12.	(a)	smart	riko	(rikɔː)
	(b)	dull	baka	(bʌkʌ)
13.	(a)	high	hikui	(hikui)
	(b)	low	takai	(tʌkʌi)
14.	(a)	kite	hune	(hune)
	(b)	boat	tako	(tʌkɔ)

15.	(a)	old	wakai	(wʌkʌi)
	(b)	young	oitaru	(ɔitʌru)
16.	(a)	hot	atsui	(ʌzui)
	(b)	cold	samui	(sʌmui)
17.	(a)	are	nai	(nʌi)
	(b)	are not	aru	(ʌru)
18.	(a)	blue	ao	(ʌɔ)
	(b)	yellow	kii	(kiː)
19.	(a)	thick	atsui	(ʌzui)
	(b)	thin	usui	(usui)
20.	(a)	big	chiisai	(tʃiːsʌi)
	(b)	small	ookii	(ɔːkiː)
21.	(a)	clear	nigotta	(nigɔttʌ)
	(b)	muddy	sunda	(sundʌ)
22.	(a)	enemy	mikata	(mikʌtʌ)
	(b)	friend	teki	(teki)
23.	(a)	crooked	magatta	(mʌgʌttʌ)
	(b)	straight	massugu	(mʌssugu)
24.	(a)	right	ayamareru	(ʌyʌmʌreru)
	(b)	wrong	tadashii	(tʌdʌʃiː)
25.	(a)	sharp	nibui	(nibui)
	(b)	dull	togatta	(tɔgʌttʌ)

this successfully it is only necessary to avoid artificial terminological schemes such as associating sounds, letters, length of word, etc. At least 75% of these pairs can be correctly guessed. It is advisable to enunciate the Japanese words as clearly as possible, not to get the sound for purposes of association but because such articulate movements are conditions favorable for the emergence of the corresponding functional organization. The phonetic system is the familiar one of the International Phonetic Association with the exception that *z*=*ts* as in ca*ts* and that *r* is pronounced half-way between the normal English *r* and *d*. The key to the correct pairing is given below the list.

The following letters signify the Japanese words which correspond to the (*a*) words of the English pairs: 1 *b*, 2 *b*, 3 *a*, 4 *b*, 5 *a*, 6 *a*, 7 *a*, 8 *b*, 9 *b*, 10 *a*, 11 *b*, 12 *a*, 13 *b*, 14 *b*, 15 *b*, 16 *a*, 17 *b*, 18 *a*, 19 *a*, 20 *b*, 21 *b*, 22 *b*, 23 *a*, 24 *b*, 25 *b*.

A proposed explanation of the phenomenon follows: The perception of a word which is known to be meaningful but whose meaning is unknown is conditioned by organized processes. Many, if not all, organized processes have specific causal properties of varying degrees of similarities. Now organized processes are also the conditions for the perception of all objects and for the experience of all meanings. Hence, when a word is "selected" to convey a meaning, the perception of that word and the thinking of the meaning will tend to give rise to organizations which have similar causal properties unless some terminological scheme is employed.

This explanation is a specific application of the theory that meanings, psychologically, are properties of natural organized wholes or processes.

The procedure employed in this experiment suggests a method which may be of value for the investigation of meaning and the origin of words and meanings.

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EDWARD L. THORNDIKE (and the Staff of the Division of Psychology of the Institute of Educational Research of Teachers College, Columbia University). *The Fundamentals of Learning.* New York: Bur. Publs., Teach. Coll., 1932. Pp. xvii+638.

The concept of learning runs with a rare pervasiveness through most of the categories of psychology and a solution of certain major problems of learning would set a new milestone in the development of psychology toward maturity. Whether, therefore, one agrees with their point of view or not, serious attempts to find solutions of fundamental problems must be acclaimed as of the first importance. There is scarcely room for doubt that in this volume Professor Thorndike has made such an attempt.

He is concerned "with the fundamental facts of learning whereby a situation which first evokes response A later evokes response B, different from A" (p. 1). Through 104 separate experiments, in terms of data presented in 153 tables, he seeks and arranges these facts. This blizzard of data pyramids to mountainous proportions by the time the last experiment and table at the end of the 145 pages of the tenth and terminal appendix, which follows the 482 pages of the volume proper, have been reached.[1] A critical examination of each experiment and of its interpretation would require a book at least a fifth as large as the one under review. It will be necessary here to describe as generally and as typically as may be and then to inquire in like manner whether by his giant piling of Pelion upon Ossa the sought-for abode of the gods has really been reached.

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[1] This book presents the detailed evidence upon which Professor Thorndike's Messenger Lectures were based, together with a more complete discussion of many theoretical points. The lectures were publishd as *Human Learning,* N. Y., Century, 1931. *Cf.* a review by the present writer in *Psychol. Bull.,* 1931, **28,** 467-477.

The experiments were obviously planned to examine learning in terms of Thorndikian system, although many of the concepts used are modifications of earlier ones or are wholly new, while the obtained facts add much to the system. It was, indeed, part of the experimental program to seek for facts hard to incorporate into such a system or irreconcilable with it. The results and their interpretation take "the form of a new associationism, or better, since it differs deeply and widely from that older British associationism, of a new connectionism" (p. 4).

Three of the basic concepts of the system are early defined, although "what we have found and stated will be equally true and useful by any reasonable definitions. The terms situation and response may continue as crude but convenient names for some state of affairs without or within the organism and some sequent act or condition of the organism, which is, or seems to be, determined in part by said state of affairs" (p. 18). "That a connection $S_1 \rightarrow R_1$ exists in a certain organism means in this study simply that there is a probability greater than an infinitesimal that, if S_1 occurs, R_1 will occur" (p. 19). The strength of a connection is the probability of the $S_1 \rightarrow R_1$ sequence.

The experiments seek first to discover the influence of the repetition of a situation. The repeated situations are strips of paper, the lengths of which are to be estimated; the idea "draw a 2-inch (or 4-, 6-, 8-inch) line"; the idea "draw an angle of 20° (or of 45° or of 60°)"; a heard word, the response to which is any digit from 0 to 9; the sounds of various letters and combinations of letters which the subject is to represent by letters; two or three letters to which other letters are to be added to make a word; and hearing nonsense syllables, each followed by a number from 0 to 9, after which the syllables alone are presented and a number is to be supplied. In some of the experiments one subject only is used; in others a considerable number are employed. A large number of records are, however, uniformly available. It is concluded from the 28 experiments upon this problem that the results as a whole, and with substantial uniformity, prove that with repetitions of a situation the frequent and strong connections gain very, very little, if at all, from the weak. The most reasonable explanation of them is that the gain is zero" (p. 62).

In this connection Professor Thorndike makes a statement which is typical of the other experiments as well. "Against any one of our experiments by itself alone certain objections can be made by believers in some theory of 'drainage,' and by disbelievers in the potency of effect, but some other of the experiments will be found free from that objection" (p. 62). The critic shuttles back and forth from one experiment to another, faced, in an effort to evaluate them as a composite, with Alpine data, finding his objections to one answered in another, until he is convinced, among other things, of the necessity for caution. While possible replies to many of the criticisms can readily be constructed, there are certain ones which apply to blocks of several experiments and which, in the interests of clarification of fundamental issues, are worth making, even though one can think of significant answers.

The situations of many of these experiments were presented, either with no instruction to learn or with instructions such as to listen passively. It has long been known that frequency of stimulation with no set or intent to learn is apt to be unproductive of learning and long experiments to prove it seem superfluous. To this the reply can scarcely be made that the introduction of intent to learn implies also effect, a condition which it was desired to eliminate from these experiments, because a subject may have a set to learn and still be kept in ignorance of his success or failure. The set-up of such experiments is arranged to measure incidental learning only, and there are other aspects which would make incidental learning very difficult. These aspects may now be discussed. In each experiment there were several different situations, sometimes a great many, and a large number of occurrences of each in a random or near-random order. The conditions were such as very often to favor the operation of associative, reproductive, and retroactive inhibition in amounts sufficient to mask the appearance of learning. There was, for example, opportunity for the same situation to elicit several different responses at different times, and, between any one such connection and the next opportunity for its occurrence, many other connections between similar items had been made. The experiments amount to one or more presentations of very long lists in which pairs of items recur with different frequencies, but there is often more opportunity for inhibition than is usually present in long lists. The conditions are such as to mask very greatly any strengthening which may have been present. In some experiments, to which this criticism does not apply, the responses were not readily available. It is not the purpose of these statements to urge that frequency of a situation strengthens connections or selects, but only that the conditions of many of these experiments do not afford clean-cut tests of the possibility.

The group of experiments just described deals with the repetition of a situation when response may be variable. The conclusion is reached that frequency has no selective power. When, however, a total connection from stimulus to response is repeated, the result might be different. There has been in psychology an almost universal belief in the potency of repetition of connections, but in most cases of repetition more or less satisfying after-effects appear. Thorndike has sought to discover what happens when the operation of such after-affects is excluded. In two experiments subjects were read sentences and then asked questions concerning sequences of words within sentences or from one sentence to the next. In another, pairs of words and two-place numbers, so arranged that certain sequences occurred frequently, were read to the subjects. In a fourth, certain kinds of words were always shown on cards of a given size and held at a given height above the desk, but no instructions to look for such features or to connect them with the response were given. These experiments lead to the conclusion that mere sequence in time, in and of itself, does very little. Nor does attentiveness to the sequential items have strengthening power unless there is attentiveness with *belonging*. For learning, responses must be made *to* situations or elements thereof, not simply come *after* them. At its simplest, belonging implies only "this goes with that" (p. 72). There is nothing logical or inherent about it.

There follows a series of experiments upon connection with belonging. One, styled "nearly crucial against those theorists who have declared that contiguity has zero power" (p. 96), may be described. University students wrote at dictation a long series of 5-place numbers, of varying frequency, read in two parts, as 874 53, and were then required to supply the 2-place number which went with each presented 3-place number. Questions were asked about certainty, associative aids, and frequency of occurrence of certain numbers. This method is used to measure "the force of sheer frequency plus belonging with as near a minimum of satisfying after-effect of the connection as is perhaps attainable" (pp. 95-96). The higher frequencies yield clear learning. Other experiments show that diminishing returns do not begin to appear until a considerable strength has been reached. Learning under conditions of close sequence with belonging is real but very slow.

The next group of experiments, using paired associates mainly, support a tentative "law of the influence of the impressiveness of the first term upon the strength of the connection leading from it" (p. 144). Impressiveness of the first member, "whether attained by the quality of the word or by repeating it" (as in the sequence *marble marble—61*) "increases the strength of the connection leading from it to its belonging sequent" (p. 137), but this strengthening does not extend beyond the belonging sequent. Impressiveness in the second member, obtained by repeating it, strengthens the connection leading to it only slightly. It is proposed to leave the concept of impressiveness vague and undefined, but this lack of definition seems to the reviewer to make difficult an evaluation of the data. Words such as *love, kiss, insane, vomit,* which are said to be more impressive than *paste, crude, aim, neglect,* may differ from them in that they have a more varied meaning, have more potency to arouse affective or emotional responses, are better fixated, or are better able because of these factors to resist interference. Impressiveness may mean one or more of these or it may mean something else. In any case, the experiments tell us that certain words enter into connection better than certain other words, but the conditions which determine the fact are not known. To label an unknown set of conditions 'impressiveness' tells nothing more general than the specific results from specific words. Much the same is true when 'impressiveness' is gained by repetition of a member.

Three brief chapters present evidence concerning distribution, polarity, and the influence of repetition upon the omission of intermediate terms. The same general conclusons which have been arrived at by work with large units of material are found to hold for single connections learned either by repetition alone or with very little operation of effect, and with all of the time intervals very short. The polarity or unidirectional character of mental connections, whereby the connections lead forward as formed rather than unify into totals beyond the laws of connection-forming, is considered strongly supported. The evidence that intermediate terms do not drop out with repetition has in part been published elsewhere and has aroused discussion.[2] Thorndike here takes account of Hollingworth's criticisms.

Frequency, even with belonging, leads but slowly to learning. Some other factor or factors must be invoked to account for learning as it actually occurs. Most potent of such factors is effect, the strengthening operation of which seems to Thorndike "as sure as the fact of learning itself" (p. 172). The old law of effect, the statement of which will follow the name of the law with high probability in the reaction patterns of every psychologist, Thorndike does not now accept in its 1913 form. "Its suggestion that the action of annoyers is the opposite of that of satisfiers in all respects is misleading" (p. 176). The new factor of belonging must be considered. Two other facts must also be emphasized: (1) "satisfyingness and annoyingness are not synonyms for sensory pleasure and pain" and (2) "the act which is the second term in the connection need not be satisfying or annoying because its after-effects are" (p. 176). The two essential features of the satisfiers which the law names are "the assertion that certain events which occur after a connection has operated can work back upon it to strengthen it, and the assertion that satisfying events are the kind which have this power" (p. 177).

We come, then, to experiments which are designed to prove and explicate the power of effect. Subjects were asked to estimate the lengths of strips of paper or to draw lines both with no knowledge of results and with statements of "Right" and "Wrong" after each judgment or drawing; to throw balls at a target and to exert specified pressures with a dynamometer, both with and without knowledge of success or failure. In these experiments effort was made to control frequency, recency, congruity, finality, and related factors, to keep the subject from learning anything about the response save that it was or was not satisfying, and to prevent the occurrence of anything which would differentiate one connection from another before or after the effect itself. They concur in showing that after-effects in the form of "Right" and of knowledge of results favor learning with emphatically greater vigor than does repetition without effect.

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[2] Freiberg, A. D., Dallenbach, K. M., and Thorndike, E. L., The influence of repetition of a series upon the omission of its intermediate terms, *Amer. J. Psychol.*, 1929, **41**, 637-639. *Cf.* Hollingworth, H. L., On the omission of intermediate acts, *Amer. J. Psychol.*, 1930, **42**, 457-458; and Dallenbach, K. M., A reply to Dr. Hollingworth, *Amer. J. Psychol.*, 1930, **42**, 458-459.

This conclusion at once elicits the question of the mode of operation of the effect. One possible answer is that the effect leads the learner to repeat or revive either the connection itself or some substitute for it. In a series of 22 experiments the 'right' response was frequently coupled with some adventitious feature to which there was little reason to expect the subject to pay attention, such as the right end of a line of multiple choices for the correct meaning of a word. The results of these experiments and related considerations "prove that a satisfying after-effect of a connection can and generally does strengthen that connection directly, irrespective of repetitions or rehearsals or recalls of the connection and of images or other representations of the after-effect" (p. 270). It should be noted that the items to which the effect really applies in these experiments are mainly such as to be unnoted by the subject, and that such learning as occurs is of the order of incidental learning. Whether the 'effect' is the only condition operating in such cases is hard to determine. The results show, moreover, only that learning may occur when the learner is unaware of the specific response to which the effect attaches. They do not prove that effect may not operate in less incidental cases by means of revival of the connection or of a substitute for it. Professor Thorndike believes, however, that the actual learning of both animals and human subjects occurs under analogous conditions and he presents convincing evidence for this belief. How effect operates is not known, but in Chapter 12 some interesting speculations are advanced and some important experimental problems are suggested.

In Chapter 11, on rewards and punishments, evidence is offered for the conclusion that annoying after-effects have no uniform weakening influence on connections and that when annoying effects do weaken, they act indirectly in that they lead the subject to make some other response to the situation and thus to render him less likely to follow the original connection. Satisfiers and annoyers are not, then, dynamic opposites.

Psychologists have been prone to ignore the factor of *readiness,* a factor which Professor Thorndike holds to be too important to neglect. He brings to bear certain observations from refractory phase, fatigue, neurasthenia, and varying excitabilities, which support the conclusion that "every connection has at any given time not only a certain strength but also a certain readiness." In such cases as "the learning of interests, attitudes, and motives readinesses are a large fraction of what we learn" (p. 338). Important, also, are the *identifiability* of situations and the *availability* or get-at-ableness of the responses. Much of learning consists in changes in these two factors. Identifiability may, for example, be gained by analysis of situations whereby they are reduced to features easier to grasp and retain, or by association with something more easily identifiable than the situation itself.

Availability may consist in exciting the response, analyzing it out, connecting it with some available cue, or constructing it out of available elements.

The situations, responses, and connections which the foregoing experiments have employed, relatively complex as they are, are still simple in comparison to what human beings actually learn. Thorndike asserts this emphatically in passing from the action of simpler conditions to that of mental systems. At least four varieties of such systems might be important: sensory, instinct, customary, and transcendental systems. In an effort to examine these possibilities some very interesting and important adaptations of the free association technique have been used. Some evidence is obtained of affiliation of impressions within a given sensory modality, but the power of attraction within a sensory field is not great. Systems organized about instinctive activities are probably few. The important systems are those created by repetition and effect, i.e., customary or habit systems. Transcendental systems have a strong case in opposition or contrariety. If a tendency of words or ideas to evoke their opposites can be explained by repetition and reward, Thorndike believes it unnecessary to go further. His examination of associative responses by opposites, however, reveals no evidence for the potency of contrariety *per se,* nothing irreducible to habits and customary systems. Further analysis of associations supports the same conclusion.

A chapter on desires and purposes deals briefly with the work of others and enunciates certain propositions. The connection which a situation makes is said to depend "upon the person in whom it is made" (p. 392). Permanent or temporary "set" may be very potent. Satisfiers or annoyers may often prove the existence of purpose and "there is no conflict between a reasonable connectionism and a reasonable purposivism" (p. 399). "No sensible connectionist believes that a man's mind is utterly subservient to the connections leading from the external stimulus of the moment. But no sensible critic of connectionism should suppose that a man's mind can fulfill its purposes and attain its desires by any other way than the operation of its fund of connections" (p. 399). This Thorndike admits to be sheer assertion and emphasizes the need of experimental fact about it.

The treatment of associative shifting and the conditional reflex is brief and adds little experimental fact. There is said to be difficulty both in explaining the phenomena of conditioning by the general principles of learning and in the reverse direction of explanation. "The C-R phenomenon seems much less general than ordinary learning" (p. 411). The discussion here is too general to convince confirmed conditional responsers. After a chapter on "Minor Experiments, Comments, and Suggestions for Further Investigation," in which a number of important questions are raised, he passes on to consider "Adverse Evidence and Arguments." Here the views of Watson, Lewin, Van der Veldt, Carr, Woodworth, and others receive comment. The substitutes for the Thorndikian factors which these men have offered suggest to him occasional features of learning rather than fundamental causation. There exist for Thorndike, he asserts at the end of this chapter and of the text proper, two real difficulties. "One is to understand how there can be states of affairs which an animal does nothing to avoid, often doing such things as attain and preserve them, and other states of affairs which an animal often rejects or avoids or changes so far as his repertory of behavior permits. The other is to understand how a connection can be strengthened by any force. Imaginary difficulties should not be added" (p. 482). "The Law of Effect in learning demands nothing more from satisfiers than their existence, and nothing more from connections than that they leave some trace which is then and there modifiable so as to increase the probability of the action of the connection in question in the future" (p. 482).

The complexities of learning emerge, thus, by virtue of repetition plus belonging and effect, while impressiveness, readiness, identifiability, availability, set and system are potent collaborators. All are aspects of a connectionism in which each finds its place and beyond which nothing mental seems to be.

The modern edition of connectionism receives in this volume a comprehensive statement, but most of the experimental results are important in their own right. The experiments are of Professor Thorndike's own devising. Many are brilliantly conceived. Many are open to severe criticism. But dominant over all details is the impression which the book gives of a solid and thorough and honest attempt to arrive by quantitative measurements at the fundamental laws of learning and, through them, at a theory of mental organization. Psychology needs such work. Let every idea expressed in this book be wrong. It would still be important. New issues are defined, old concepts are set in a wider context, experimental problems by the score leap from it. It would be too much for any author to hope that constructions as general and as far-reaching as these could escape criticism and modification. Neither should dim the light which the book sheds on learning.

The experiments themselves are so numerous as to forbid, in this context, the detailed examination which they deserve. Through a great many of them runs, however, one aspect which interpretation must consider. "Set," while admitted, has usually been uncontrolled. Either the subjects were left to "set" themselves or, at best, they were instructed to be passive or to pay ordinary attention. What self-instruction the subjects imposed is unknown. Hard to control, in many experiments upon learning uncontrolled, "set" is peculiarly important in Thorndike's experiments. In some, conclusions have rested upon small differences, smaller than the importance attached, particularly to effect, would lead one to anticipate. The size of the differences may well be a function, however, of the fact that learning was either incidental or done without the establishment of a specific intent to learn. The experiments are, moreover, while numerous, concerned with a relatively narrow range of materials and the conclusions are generalized far beyond these materials.

Many of the experiments were planned to test connectionist principles and the results have been held to support a connectionism. It is peculiarly important, therefore, to look beyond the experimental facts to the concepts under which they are subsumed.

When we turn to the conceptual structure of the experiments, and to their interpretation, a lack of psychological analysis of the fundamental concepts is at once apparent. They are not, to borrow Professor Thorndike's own term, sufficiently *identifiable.* A *situation* may be any antecedent to which a relatively brief and unitary designation may be given and varies from "Draw a 2-inch line" to any indeterminate degree of complexity. A *response* may be anything the subject does as a result of the situation while a *connection* is the probability that a response will follow the situation. The Thorndikian system aims at specificity, at identifiable and indicatable phenomena, at simplicity of units and away from complex wholes and conditions not open to precise measurement. Yet in addition to the unanalyzed and vague character of its three fundamental terms it gives us basic conditions of connection which are equally indeterminate and complex. To belonging, impressiveness, effect, identifiability, and availability little specific definition is given. That these words correspond to important conditions the data assure us, but what these conditions are nothing in the 104 experiments has told us. This is not to say that these huge arrays of measurements which Professor Thorndike has made are husks in the guise of bread. As they stand, the measurements are valuable. They would be more valuable still if the basic terms were more precisely analyzed. Only then can critical systematizing be done.

The ubiquity and, withal, the looseness of the terms shout unrestrainedly from the following quotation. "Connections lead from states of affairs within the brain as well as from external situations. They often occur in long series wherein the response to one situation becomes the situation producing the next response and so on. They may be from parts or elements or features of a situation as well as from the situation as a whole. They may be largely determined by events preceding their immediate stimuli or by more or less of the accompanying attitude or set of the person, even conceivably by his entire make-up and equipment. They lead to responses of readiness and unreadiness, awareness, attention, interest, welcoming and rejecting, emphasizing and restraining, differentiating and relating, directing and coordinating. The things connected may be subtle relations or elusive attitudes and intentions" (p. 353). All of these things may be, but what they mean analysis has yet to reveal. Here is blithe promise of a complete system, but the system is not here. At times one almost fears that Professor Thorndike believes the promise to be fulfilled, but in reassuring asides there sometimes appears insightful humor which ungrounds one's fears. To a game defense of a theory for all the evidence will bear, or even for a little more, so long as a slightly sardonic smile at himself plays on the face of the defender, one can grant much which to the smug apostle of his own bound and delivered "truth" one would deny.

The fact of *"belonging"* has the promise of high systematic importance, but to use it even in as simple a form as the meaning "this goes with that" is both to use a term of uncertain limits and to invoke a concept for which the connectionist system has hardly paved the way. It smacks of Würzburg more than of New York. By what principle, moreover, does belonging act upon connections? For whom or for what do terms belong? Is belonging identifiable and definable prior to any learning of the connection? Clean-cut definitions and answers to such questions would go far toward making the results more interpretable.

Effect is, likewise, unanalyzed and unspecified. The major effects which followed the acts to be learned were the spoken words "Right" and "Wrong." These are far from being simple satisfiers and annoyers. The ability of the word "Right" to satisfy or to have whatever effect it does have, depends upon a context built complexly in the subject's past. To separate "Right" from its context in the individual system is justifiable experimentally, but whether the obtained results may legitimately be used to support a connectionism needs proof. The residues, the generalized meanings and sets and attitudes, which develop within the organization of the individual and which must have been represented in the subjects' interpretation of "Right," have neither been shown to be explicable in terms of current connectionism nor to act in any direct and simple way upon connections. The operation of the wealth of mental events which such a word in an experimental setting evokes in an educated adult may well reach beyond simple statement. Certainly psychological analysis is needed before the experimental results obtained can be made an important part of a system of mental organization. What, moreover, is satisfyingness and what is its relation to the spoken word "Right"? It is obviously not an affective state only. May it sometimes be, or may it always partly be, affective? Is it a meaning? If so, what is meaning to connectionism?

Complexly interwoven as the conditions of these experiments are, they are simple and deliberate artifacts compared with the acts which comprise the major armamentarium of the laboratory investigations of learning, not to mention the things which human beings learn in the course of life. This Professor Thorndike emphatically asserts. But in such activities as the more complicated skills of the laboratory and the acquisitions of daily life, to what does effect apply? Does it apply when it acts at all, to each specific connection involved? If it does, it seems to be a less potent and ubiquitous condition of learning than he would consider it. In complex acts the learner often fails to identify the varied "right" components of the act, although the total act is "successful" and although the component "right" acts are fixated. When the "right" part-act is not identified, it is difficult to understand, on Thorndikian principles, how the positive effect of the series of acts could affect the part-act. This is a matter which he at least leaves untouched. It may be held, of course, that the effect of the whole series operates upon each part of the series, but such an assertion would be hard to understand on the principles under discussion.

It is repeatedly made plain, however, that the results of these experiments are believed to be applicable to the most complex integrations. This may be true, but it may also be that in the learning of complex activities the interserial and intra-serial influences are so great as to require additional concepts to explain them. Certain it seems to be that defensible generalizations from the materials of these experiments to much more complicated ones are more tentative in character than Professor Thorndike's confident statements would imply. In the same way the mind's set, the individual, the person, are said to influence connections and their terms. The gulf between the factors which these experiments aim to measure and the influence of the total individual or even of the mind's momentary set is wide and not to be crossed by cheerful admission that the other side is there. At the brink, the system is courageous but, thus far, baffled.

Throughout the book one wonders what is the existential status, what are the elementary descriptive properties, of the fundamental terms. Qualities, relations, attitudes, activities of the mind, purposes, are, furthermore, used easily and without apology. What, on connectionist principles, is the status of qualities or of relations? Are purposes themselves connections or situations or responses? In a quotation cited above Thorndike speaks of the mind fulfilling its purposes by the operation of connections. Here purposes seem to be set off from any of the fundamental terms of the system. At times, of which this is one, Thorndike comes perilously close to admitting into the system concepts difficult to reduce, and certainly not yet reduced, to the tenets of the system.

The nervous system as an explanatory principle plays a relatively smaller part in this book than it did in *Human Learning*. There the doings of the neurons were frequently invoked; here they enter incidentally, when at all, although beyond the connections, whose existence and strength are known and measured as probabilities, there lies the steady implication of neural connections.

One would have welcomed a more extended and critical account of the criticisms which have been leveled against connectionism and of experiments hard to reconcile with it. Thorndike seems to consider the views of others, but infrequently in critical detail. The great wealth of experimental and theoretical work directly relevant to his own experiments receives scarcely more than a courteous gesture and a hasty passing by on the other side. It is clearly not the intent of the book to summarize the work of others and the criticism that such work has been lightly treated would be pointless did not the book seem to consider other work more completely than it really does and to erect a more complete and critical system than one actually finds in it.

No criticism should, however, be allowed to obscure in the slightest the fact that the book is a strong and vigorous attempt to solve important problems. It aims to build on quantitative measures, not on speculation. It is a major contribution to the field of learning and by such serious major researches psychology may hope to mature.

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